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# ANALOGY IN SCIENCE[1]

ROBERT OPPENHEIMER

*The Institute for Advanced Study*

PRESIDENT NEWCOMB, members of the American Psychological Association, ladies and gentlemen:

I listened to what President Newcomb just said to explain why I had been asked to speak here; but it did not clear it up for me entirely. I have thought about this question ever since, in a response to the honor and pleasure of being asked to come, I accepted. I wondered then what a professional physicist should be doing on this platform, a somewhat old and not so lively professional physicist, but still that.

I thought first that your inviting me might have a relation to the Institute for Advanced Study. We have had there a few members who are psychologists; we have an advisory committee consisting of members of this organization, whom we love and trust; and we hope to continue this. I have learned a great deal from them; we have often become friends. But we do not have a program in psychology; we do not have a department of psychology. We have, in fact, only two schools. One is called the School of Mathematics and the other called the School of Historical Studies; and it may help to reveal the limitations of my own scope if I describe very briefly what these are. You may recognize blind spots in me which will be comforting later, as I get on with my talk.

The School of Mathematics and the School of Historical Studies have both, of course, the problem of filtering the immense, fascinating, inchoate, unmanageable complexity of our experience. But they filter in quite different ways. The School of Mathematics is concerned with relations, with forms, with logical structure, and the application of these patterns and their discovery to the empirical sciences. And so it happens that psychologists are members of the School of Mathematics.

The School of Historical Studies uses a different kind of filtration. When I was in England not long ago I talked to Namier, who has undertaken the compilation of the parliamentary biographies of all Members of Parliament, from the origin to now. In the first parliaments almost nothing is available in the record about most of the members, so it is hard to write about this. And at present it is hard to write the biographies because there are such volumes available about everybody; only in the 16th and 17th and 18th centuries is the amount of material fit for human compass. The filtration of history is, of course, a very special one. It not only reduces the volume of available evidence and experience; it does so through the eyes of once living people who, by their actions, their evaluation, their tradition, have selected the things which are to remain meaningful over the years.

It is very often in history that just the unique point, the point that has no satisfactory, exhaustive, formal relation to more general patterns, is what is interesting. In the School of Mathematics it can only be things general enough so that structure can be recognized. I need to add that one mathematician, who has made such great contributions to logic, Gödel, has said of mathematics that it is purely an historical accident that it developed along quantitative lines. This, which is one of the themes which I take as text for today, may moderate somewhat the austerity of the two schools of learning. Yet taking it all-in-all I can only describe the relations of the Institute to psychology by a story.

About twenty years ago for the first time I visited the great laboratory in New York where Professor Rabi and his colleagues were beginning to do the most exciting experiments on molecular beams; and I had a fine time. But, as I left, I noticed that over the door it said in somewhat dusty letters, "Cosmic Ray Laboratory," and I asked Rabi, "What the heck?" "Well," he said, "you see, we don't keep them out."

I have thus given up talking about the Institute; and my second thought is rather simpler. It is to say a few things about physics which are, I think, interesting and which, I hope, may be helpful if not taken too literally and too seriously, also in the various fields of psychology. I know that it is a terrible bear trap to talk of the philosophy of

[1] Presented at the Sixty-Third Annual Meeting of the American Psychological Association, San Francisco, California, September 4, 1955.

science; only in a very, very limited sense am I going to do that.

One would think that the two sciences could hardly be further apart. In all hierarchical schemes they are put far apart. Psychology, to everyone who works in the field, is felt to be a new subject in which real progress and real objectivity are recent. Physics is, perhaps, as old as the sciences come; physics is reputed to have a large, coherent, connected corpus of certitudes. This does not exist in psychology, and only the beginnings of it, the beginnings of things that are later going to be tied together, are now before us.

But I have always had a feeling that there were ways in which the two sciences had a community; in some sense, of course, all sciences do. One very simple one is that each is responsive to a primitive, permanent, pervasive, human curiosity: What material bodies are and how they behave, on the one hand, and how people and the people-like animals behave and feel and think and learn. These are the curiosities of common life and they will never be abated. Both, for this reason, can hardly make important pronouncements of a technical sort which do not appear to have some bearing on our views of reality, on metaphysics. Both manifestly have, and continue to have, a fresh and inspiriting effect on the theory of knowledge, on epistomology.

There are other ways in which we are brothers. In the last ten years the physicists have been extraordinarily noisy about the immense powers which, largely through their efforts, but through other efforts as well, have come into the possession of man, powers notably and strikingly for very large-scale and dreadful destruction. We have spoken of our responsibilities and of our obligations to society in terms that sound to me very provincial, because the psychologist can hardly do anything without realizing that for him the acquisition of knowledge opens up the most terrifying prospects of controlling what people do and how they think and how they behave and how they feel. This is true for all of you who are engaged in practice, and as the corpus of psychology gains in certitude and subtlety and skill, I can see that the physicist's pleas that what he discovers be used with humanity and be used wisely will seem rather trivial compared to those pleas which you will have to make and for which you will have to be responsible.

The point, of course, is that as the relevance of what we find to human welfare and human destiny becomes sharper and more manifest, our responsibilities for explication, for explanation, for communication, for teaching grow. These are rather our responsibilities for being sure that we are understood than responsibilities for making decisions; they are our responsibilities for laying the basis in understanding for those decisions.

There are other ways in which we are alike. The practical usefulness of our professions gives us often the impression that we are right for the wrong reasons, and that our true nature is very different from our public presence. We are both faced with the problem of the need to keep intact the purity of academic and abstract research and, at the same time, to nourish and be nourished by practice. In physics, of course, our debt to technology and engineering is unlimited. I think it would be so in psychology as well.

Both sciences, all sciences, arise as refinements, corrections, and adaptations of common sense. There are no unique, simple, scientific methods that one can prescribe; but there are certainly traits that any science must have before it pretends to be one. One is the quest for objectivity. I mean that not in a metaphysical sense; but in a very practical sense, as the quest to be sure that we understand one another, and that all qualified practitioners mean essentially the same thing. Common-sense language is inherently ambiguous; when the poet uses it, or the rhetorician, he exploits the ambiguity, and even when we talk in ordinary life we almost need ambiguity in order to get by. But in science we try to get rid of that, we try to talk in such simple terms, and match our talk with deeds in such a way that we may differ as to facts, but we can resolve the differences. This is, of course, the first step in the quest for certitude. But certitude is not the whole story. When we move from common sense into scientific things, we also move toward generality using analysis, using observation and, in the end, using experiment. And we also do something which is even more characteristic; we look for novelty, we look for transcendence, we look for features of experience that are not available in ordinary life. Characteristic in physics are the instruments that enable us to transcend elementary, daily experience: the telescope that lets us look deep into the sky, the enormous accelerators which are, today, the logical extension of the microscope, enabling us to look on a finer and finer scale into the structure of matter.

I need to be cautious in citing parallels in psychology; but certainly the use of hypnosis, the use

of drugs, are typical extensions into unfamiliar realms of human experience which just bring out characteristics of psychological phenomena that are largely lost in day-to-day experience. There is an example which may be only a physicist's idea of a perfect experiment. It is the work that was done at McGill in the last years on the effects of reducing sensory stimuli, with very simple arrangements to change the level of stimulation; these produce most striking and almost frighteningly great, though essentially temporary, changes in memory, in the intellectual and cognitive life of the subjects. This is again an example of carrying to an extreme something which is indeed encountered in ordinary experience but which only the patience and the abstractness of experimental enquiry is likely to make manifest.

We come from common sense; we work for a long time; then we give back to common sense refined, original, and strange notions, and enrich what men know and how they live. And here, I suppose, the real hero is the teacher.

I chose as my theme, "Analogy in Science." What I am going to talk about is analogy as an instrument in science and, to a much lesser extent, some slight traits of analogies between the sciences; mostly the second theme has led to misunderstanding and limitation; as for the first theme, analogy is indeed an indispensable and inevitable tool for scientific progress. Perhaps I had better say what I mean by that. I do not mean metaphor; I do not mean allegory; I do not even mean similarity; but I mean a special kind of similarity which is the similarity of structure, the similarity of form, a similarity of constellation between two sets of structures, two sets of particulars, that are manifestly very different but have structural parallels. It has to do with relation and interconnection. I would like to quote you a scholastic comment on analogy. It is a translation of Penido, "In a very general sense every analogy presupposes two ontological conditions; one, a plurality of real beings and thus among them an essential diversity. Monism is the born enemy of analogy. And, two, at the very heart of this multiplicity, of this inequality, a certain unity."

It is a matter about which we could argue whether these structural elements are invented by us, or whether they are discovered in the world. I find it very artificial to say that they are invented, in the sense that they are more of an artifact than the particulars which they unite and describe. I may tell one incident in the long history of astronomy and physics, which makes this very vivid for me. For practical purposes, for prophecy and ritual, the Babylonians worked out a method of predicting what days the moon would first be visible, of predicting lunar eclipses and certain rarer astronomical events. They did this by purely mathematical methods. They observed when things happened, and they got the pattern of it. They were very good. They got so good that their methods were in use in the last century in India to predict eclipses within some thirty minutes, using these two thousand year old methods. The Babylonians not only became very good, but they enjoyed it very much and they did it for fun; long after the practical reasons had gone away they published these tables, apparently as we publish articles on the internal constitution of the stars, because it is interesting. They did all of this without any celestial mechanics, without any geometry; nothing moved; there were no objects circulating around in orbits; there were no laws of motion; there was no dynamics; this was just in the field of the numbers.

You know how today we predict eclipses and first risings. It would seem to me very wrong to pretend that the mathematical regularities which were the basis of the Babylonian predictions were something they invented; it would seem to me equally wrong not to recognize in celestial mechanics as we now know it, a far deeper and more comprehensive description of regularities in the physical world. I think that not only because it is a little more useful, I think that not only because it unites more subjects, but because it reveals an aspect of the regularities of the world which was wholly unseen by the Babylonians.

Perhaps I need now to quote from Charles Peirce, and get on: "However, as metaphysics is a subject much more curious than useful, the knowledge of which, like that of a sunken reef, serves chiefly to enable us to keep clear of it, I will not trouble the reader with any more Ontology at this moment."

Whether or not we talk of discovery or of invention, analogy is inevitable in human thought, because we come to new things in science with what equipment we have, which is how we have learned to think, and above all how we have learned to think about the relatedness of things. We cannot, coming into something new, deal with it except on the basis of the familiar and the old-fashioned. The conservatism of scientific enquiry is not an arbitrary

thing; it is the freight with which we operate; it is the only equipment we have. We cannot learn to be surprised or astonished at something unless we have a view of how it ought to be; and that view is almost certainly an analogy. We cannot learn that we have made a mistake unless we can make a mistake; and our mistake is almost always in the form of an analogy to some other piece of experience.

This is not to say that analogy is the criterion of truth. One can never establish that a theory is right by saying that it is like some other theory that is right. The criterion of truth must come from analysis, it must come from experience, and from that very special kind of objectivity which characterizes science, namely that we are quite sure we understand one another and that we can check up on one another. But truth is not the whole thing; certitude is not the whole of science. Science is an immensely creative and enriching experience; and it is full of novelty and exploration; and it is in order to get to these that analogy is an indispensable instrument. Even analysis, even the ability to plan experiments, even the ability to sort things out and pick them apart presupposes a good deal of structure, and that structure is characteristically an analogical one.

Let me read you now a few relevant and eloquent words of William James. He wrote them in one of his later accounts of pragmatism, at a time when his own good sense and shrewd observation and wisdom and humanity made him aware of the fact that to say only that an idea was true because it worked was a rather poor description of what went on in science, that something was missing from that account. This is what he wrote:

> The point I now urge you to observe particularly is the part played by the older truths. Failure to take account of it is the source of much of the unjust criticism levelled against pragmatism. Their influence is absolutely controlling.. Loyalty to them is the first principle—in most cases it is the only principle; for by far the most usual way of handling phenomena so novel that they would make for a serious rearrangement of our preconception is to ignore them altogether, or to abuse those who bear witness for them.

What I want to do next is to give you five examples of the use of analogy in atomic physics. They will not all be equally familiar; perhaps that is an understatement, for some are very new, even to such a point new that I do not know how good the analogies are and we have not yet found the decisive point at which they are mistaken.

The analogies in physics may very well be misleading for biologists and psychologists, because of the enormous part that rather rigid formal structure plays in physics. This structure is not perhaps necessarily quantitative, though in fact much of it is quantitative. Our ability to write down synoptic relations in symbolic form, our use of formulae, enables us to talk of vast amounts of experience, very varied experience, very detailed experience, in a shorthand way; and to point sharply to mistakes, to correct error on occasion by altering only one letter, that changes everything. These examples are thus not meant as paradigms, but rather as an illustration of the fact that, in what is regarded as one of the most rigorous and certain of the sciences, we use an instrument which has been in great disrepute, because uncritically used it can confuse invention with confirmation and truth.

Let me give a first example which is not from atomic physics, which is almost from pre-physics, because it deals with very familiar things and yet illustrates the nature of the role of form in the use of analogy in physics. This has to do with Jean Buridan and the Paris school of the 14th century and the theory of impetus. What was their classic view? Physics has a special meaning for the word "classic"; classic means wrong, it means a wrong view that was held to be right a little while ago. The classic view was that the natural state of matter was rest, and that where you found bodies in motion you needed to look for a cause. This was the Schoolman's view; it was Aristotle's view. It is, in fact, supported by a lot of observation. It is not well supported by observation on projectiles; the notion that air pushes the bullet becomes less plausible the more you watch. Buridan and his colleagues took a step, making a new analogy, probably the greatest step in the history of Western science. They said, it is true that matter has a natural state, but it is not rest. It is true that when it departs from this natural state this must be ascribed to the intervention of a cause. But the natural state is one of constant impetus, one of constant momentum, one of uniform velocity. And with that the beginnings of rational mechanics and rational physical science were made. This seems a small change, to replace the coordinate by the velocity; it is a small change; and yet it is a change in the whole way of thinking about the physical world.

Let me list the five illustrations from atomic physics: they are what has happened to the idea

of waves; what has happened to the ideas of classical physics in the atomic domain, the so-called correspondence principle; the analogy between radioactive decay and emission of light which we owe to Fermi; the analogy between electromagnetic forces and nuclear forces, between electrodynamics and mesodynamics; and a final subject which I will only call strangeness because that is about all I know about it.

Take the wave theory. It originated in the observation of regular, rhythmic changes in matter, waves on water, and was developed by an easily conducted physical exploration of sound waves, where there is a periodic change, a regular change in the density of air or other media. Both of these phenomena exhibit a characteristic. If two waves collide they can cancel each other out, or they can reinforce each other. They show interference. They have another abstract property: If waves pass through an orifice or around an obstacle that is small compared with the wave length, then the obstacle or the orifice does not cast a sharp image or shadow, but there are characteristic blurring effects which are called diffraction. Waves superpose; the sum of the two waves is just what you get by adding algebraically and not arithmetically; you may get zero if you add equal positive and negative waves; this again is interference.

This abstract set of properties is persistent; light is also a wave motion, but there is no matter in motion; there is no substrate. It was a great mark of progress for physics to recognize this disanalogy. There is still motion; and what moves are physically measurable things, rather more abstract things, electric fields and magnetic fields. Again we find interference, diffraction, and superposition, the same abstract characteristics, and again in principle, the infinitely regular, infinitely repeated pattern as a special case of a wave.

More extremely abstract examples are the waves of atomic mechanics, of wave mechanics, because these waves in the first place are in multidimensional space, then they are represented by complex numbers so that they are not directly measurable; they are indeed quite unobservable. There is nothing to measure in the physical world that corresponds to these waves. They are indirectly connected with observation; but they have again these same abstract properties—interference, linearity, superposition, diffraction; and when one talks about them, one uses much the same mathematics as for sound and light waves, although it is not the fact that one can use the mathematics but the fact that the structure and the relations are the same that is the decisive discovery. These waves represent, if one wants to say what they are, not matter, not forces, not electric fields, but essentially the state of information about an atomic system.

At each point the first scientists have tried to make a theory like the earlier theories, light, like sound, as a material wave; matter waves like light waves, like a real, physical wave; and in each case it has been found one had to widen the framework a little, and find the disanalogy which enabled one to preserve what was right about the analogy.

The second example of analogy is a massive one; it is, I think, the greatest experience in this century for the physicist, even greater than relativity; it is the discovery of atomic mechanics. Here again, in a way very characteristic of scientific theory, great conservatism presided over and guided the development. What is all this about? When one gets to the atomic domain, and this is a domain of small actions, of limited distances and limited impulses, of things such as one encounters in atoms and nuclei, then the coarseness of the whole physical world, its granular atomic structure, for the first time begins to manifest itself. This is not yet the granulation of the fundamental particles, but the granulation of atomic physics itself, of the quantum of action. What this turns out to mean is that when one tries to study such a system there are aspects of it which are accessible to experiment but are not compatibly or simultaneously accessible to experiment. A famous example is in the uncertainty relations, that one can determine the location of something in time and space, but if one does that, he uses an experimental setup which makes it impossible to know exactly what the impulse or velocity or energy of the system is. One may do the opposite; one can study the impulse and then lose all account of where the object is. And one can, of course, compromise with limited knowledge of both; but one cannot combine; and we call these the complementary aspects of an atomic system, and the complementary character of the fundamental observations. That means that we cannot talk about an atom as we can about a classical mechanical system. We cannot say the objects in it are here and they are moving in certain orbits and so on; in fact, in ordinary atoms there are no orbits. In atoms as they are ordinarily encountered there is something entirely different; there are stationary states which have a stability, a uniqueness,

a reproduceability, which has no counterpart in classical physics at all, which could not exist if it were not for a revolutionary new feature.

One can talk about these stationary states in a consistent way; one can describe them accurately, and predict them; but one has a vast change from the familiar experience of bodies in motion, of matter in motion. Sometimes people say that this atomic theory is characterized by the fact that we cannot observe a system without disturbing it. But that is not quite right. It is not the disturbance which makes the trouble; it is the fact that the means of observation would be frustrated as means of observation if we tried to take account of the disturbance which we are making. This is thus a slightly more subtle matter. Sometimes people say that the electron has a position and momentum but we cannot measure them simultaneously. But this is not right either, because only the act of observation, the coupling of the atom with the physical measuring equipment, makes it logically permissible to attribute a position to an electron. We cannot get the right answer by saying that the electron has a position, and since we do not know what it is, let us average. If we do that we get a wrong answer. We have to admit that unless the situation is one which is created by our physical operation on the atomic system to realize, to manifest, to objectify the localization of the electron, then it will not be localized; it will in fact have no properties at all apart from what we do to it.

All of this is extraordinarily radical and extraordinarily unlike Newtonian mechanics. But what does the physicist say? Even before the full answer was found it was said there was something going on here which limits classical ideas; they do not quite apply; but in any situation in which they do apply we know that they are right; and, therefore, whatever laws hold in the atomic domain, they must merge into the laws of classical mechanics. There must be a one-to-one correspondence, an analogy; otherwise, in capturing some insight into this new domain we will throw out all we ever knew, and throw out things that are true. This affirmation is called the correspondence principle. Let me give an example of how extremely compact is this correction of the analogy, which has revolutionized everything, of how one deals with analogy in a highly formalized science.

Each law of classical mechanics may be written so that it is true in atomic mechanics: that the velocity is proportional to the momentum; that the change in time of the momentum is proportional to the force; that the energy is conserved. All of these things hold provided we make one formal change, provided we say that the momentum and the coordinate are not numbers, but are objects such that when we multiply the momentum by the coordinate and when we multiply the coordinate by the momentum we do not get the same answer, and that the difference between these two answers is an imaginary, universal, atomic constant. If we just write that one formula, then everything we had before is formally identical with what we have now. This is not only a powerful illustration of the use of analogy and disanalogy in a formal science; it played a decisive part in the exploration and discovery of the atomic world. We shall have to come back to other aspects of this great development. Let me run rather more briefly over the three other examples.

Radioactive nuclei, almost all of those that are made artificially, and many natural ones, disintegrate by sending out electrons. We puzzled and puzzled over this, since it was quite clear that there were no electrons in nuclei. Then Fermi made the suggestion that one might describe this as one describes the emission of light or light quanta from atoms. Nobody would say there was a light quantum in an atom; but still we observe light coming out; and he made a theory along these lines. It was not exactly right; the analogy was not quite perfect; but with a very little adjustment which took some fifteen years of comparison with the details of experiment, we have a description and a theory that work fine.

The Japanese physicist Yukawa proposed a somewhat braver analogy, whose fortunes are still not entirely clear. He proposed a similarity between electrical and nuclear forces. The way in which one describes the forces between electrically charged bodies is, of course, that one charged body makes electric fields; and these electric fields are propagated to other bodies, and give them some momentum to push them around. Nuclear forces, which are not electromagnetic, but are very strong and spectacular, Yukawa said, would probably be due to a field of a new kind; replacing the electric field there would be this new field; and replacing the light quanta, there would be new kinds of particles. Using general arguments of relativity and complementarity, or quantum theory, he concluded that because the forces between nucleons are of short range these new particles would have a mass

some hundreds of times that of the electron; and from other particularities of nuclear forces, he drew conclusions about the nature of these particles. These particles were found in cosmic rays; they are called mesons. The analogy which Yukawa started with has been refined; one has discovered that there are many differences between mesodynamics and electrodynamics. One is at the present time not quite sure what all of the key points of difference, of disanalogy, are. Some of them have been discovered, but they appear to be rather the more trivial ones; yet the theory, as it stands now, has some predictive value; it has brought order and clarity to a part, at least, of nuclear physics; it has kept people at work, busy for twenty years of rather odd and arduous and rarified boondoggling. It has been a very major event in physics and I do not know at the moment how to describe what limits this analogy, why it is not a perfect one. If we were having a seminar on physics I would talk about it for an hour, but I still would not know.

The troubles, though, are probably connected with my fifth example. It is true that these mesons of Yukawa's were discovered; but not very long after that, in the last five years, one has found a whole lot of other objects—about six manifestly different objects and maybe more to come, which are also quite stable and last quite a while, and which are not simple mesons of the kind Yukawa envisaged. Almost certainly their intervention in the picture, which is not something that is provided for in the analogy we started from, will provide a clue to the new point. But, their existence raises a different problem. Whenever in physics one encounters a situation in which something does not happen, or happens very slowly, one finds it interesting; and the great point was why do these new particles not decay quickly. They do decay; but it takes them an inordinately long time, and they come apart into products which one would expect to emerge right away.

We have a great deal of experience with reactions that occur slowly or not at all; and the characteristic reason for that is that something does not tend to change, like the energy of a system, or the total charge: something is conserved. Whenever that turns up, it also turns out that the fact that something is invariant and unchanging is mathematically identical with the statement that something makes no difference to the behavior of the system. Examples of what may make no difference to the behavior of a system may be its position in space, or its orientation in space, or some more abstract circumstance. Thus the first thing that we all did was to try to find the characteristic of these new particles that did not tend to change. That has not been hard to do; and a quite successful theory has been developed which accounts for some of the great peculiarities in this field. We do not have a good name for what does not tend to change, and the inventor of it calls it the "strangeness."

These five examples are not meant to exhaust, but merely to illustrate, the powerful use, the inevitable use, of analogy in a well-developed, in a highly-organized, highly-formalized, highly-coherent science. I need to point out that in every case an immense amount of experience, of measurement, of observation, and of analysis has gone both to the correction of the analogies and to their confirmation.

When I turn to the question of analogies between sciences I talk of something very different. There is first of all the fact that there are often situations that are not analogies at all. There are congruences when, in two different sciences, by different techniques, different language, different concepts, it turns out that the same subject has been explored from two sides. And when it turns out that there is a mapping of one description on the other, usually one description contains more elements than the other, is richer; the other may then be more economical and more convenient. Examples: The chemical theory of valence and atomic physics, which are identical except that atomic physics does give an account of some phenomena, such as resonance, which were hard to cope with within the framework of the classical chemical theory. Another example, newer and perhaps not yet as well explored or understood, lies in classical genetics on the one hand, and the discovery of the genetic substances DNA, RNA, and so on, which are, at the moment, very close to being in a one-to-one correspondence, but in which the biochemical description will turn out richer, more relevant to dynamics, and more subtle.

These are great events of science; when they happen there is rejoicing, and when they do not happen there is hope. These are the great events which bring coherence and order and large structure to the unfolding of scientific life. But probably between sciences of very different character, the direct formal analogies in their structure are not too likely to be helpful. Certainly what the pseudo-Newtonians did with sociology was a laugh-

able affair; and similar things have been done with mechanical notions of how psychological phenomena are to be explained. I know that when physicists enter biology their first ideas of how things work are indescribably naive and mechanical; they are how things would work if the physicists were making them work, but not how they work in life. I know that when I hear the word "field" used in physics and in psychology I have a nervousness that I cannot entirely account for. I think that, especially when we compare subjects in which ideas of coding, of the transfer of information, or ideas of purpose, are inherent and natural, with subjects in which these are not inherent and natural, that formal analogies have to be taken with very great caution.

But for all of that I would like to say something about what physics has to give back to common sense that it seemed to have lost from it, not because I am clear that these ideas are important tools in psychological research, but because it seems to me that the worst of all possible misunderstandings would be that psychology be influenced to model itself after a physics which is not there any more, which has been quite outdated.

We inherited, say at the beginning of this century, a notion of the physical world as a causal one, in which every event could be accounted for if we were ingenious, a world characterized by number, where everything interesting could be measured and quantified, a determinist world, a world in which there was no use or room for individuality, in which the object of study was simply there and how you studied it did not affect the object, it did not affect the kind of description you gave of it, a world in which objectifiability went far beyond merely our own agreement on what we meant by words and what we are talking about, in which objectification was meaningful irrespective of any attempt to study the system under consideration. It was just the given real object; there it was, and there was nothing for you to worry about of an epistemological character. This extremely rigid picture left out a great deal of common sense. I do not know whether these missing elements will prove helpful; but at least their return may widen the resources that one can bring to any science.

What are these ideas? In our natural, unschooled talk, and above all in unschooled talk about psychological problems, we have five or six things which we have got back into physics with complete rigor, with complete objectivity, in the sense that we understand one another, with a complete lack of ambiguity and with a perfectly phenomenal technical success. One of them is just this notion that the physical world is not completely determinate. There are predictions you can make about it but they are statistical; and any event has in it the nature of the surprise, of the miracle, of something that you could not figure out. Physics is predictive, but within limits; its world is ordered, but not completely causal.

Another of these ideas is the discovery of the limits on how much we can objectify without reference to what we are really talking about in an operational, practical sense. We can say the electron has a certain charge and we do not have to argue as to whether we are looking at it to say that; it always does. We cannot say it has a place or a motion. If we say that we imply something about what we ourselves—I do not mean as people but as physicists—are doing about it.

A third point is very closely related to this; it is the inseparability of what we are studying and the means that are used to study it, the organic connection of the object with the observer. Again, the observer is not in this case a human; but in psychology the observer sometimes is a human.

And then, as logical consequences of this, there is the idea of totality, or wholeness. Newtonian physics, classical science, was differential; anything that went on could be broken up into finer and finer elements and analyzed so. If one looks at an atomic phenomenon between the beginning and the end, the end will not be there; it will be a different phenomenon. Every pair of observations taking the form "we know this, we then predict that" is a global thing; it cannot be broken down.

Finally, every atomic event is individual. It is not, in its essentials, reproducible.

This is quite a pack of ideas that we always use: individuality, wholeness, the subtle relations of what is seen with how it is seen, the indeterminacy and the acausality of experience. And I would only say that if physics could take all these away for three centuries and then give them back in ten years, we may well say that all ideas that occur in common sense are fair as starting points, not guaranteed to work but perfectly valid as the material of the analogies with which we start.

The whole business of science does not lie in getting into realms which are unfamiliar in normal experience. There is an enormous work of analyzing, of recognizing similarities and analogies, of

getting the feel of the landscape, an enormous qualitative sense of family relations, of taxonomy. It is not always tactful to try to quantify; it is not always clear that by measuring one has found something very much worth measuring. It is true that for the Babylonians it was worth measuring—noting—the first appearances of the moon because it had a practical value. Their predictions, their prophecies, and their magic would not work without it; and I know that many psychologists have the same kind of reason for wanting to measure. It is a real property of the real world that you are measuring, but it is not necessarily the best way to advance true understanding of what is going on; and I would make this very strong plea for pluralism with regard to methods that, in the necessarily early stages of sorting out an immensely vast experience, may be fruitful and may be helpful. They may be helpful not so much for attaining objectivity, nor for a quest for certitude which will never be quite completely attained. But there is a place for the use of naturalistic methods, the use of descriptive methods. I have been immensely impressed by the work of one man who visited us last year at the Institute, Jean Piaget. When you look at his work, his statistics really consist of one or two cases. It is just a start; and yet I think he has added greatly to our understanding. It is not that I am sure he is right, but he has given us something worthy of which to enquire whether it is right; and I make this plea not to treat too harshly those who tell you a story, having observed carefully without having established that they are sure that the story is the whole story and the general story.

It is of course in that light that I look at the immense discipline of practice, that with all its pitfalls, with all the danger that it leads to premature and incorrect solutions, does give an incredible amount of experience. Physics would not be where it is, psychology would not be where it is if there were not a great many people willing to pay us for thinking and working on their problems.

If any of this is true there is another thing that physicists and psychologists have in common: we are going to have quite a complicated life. The plea for a plural approach to exploration, the plea for a minimal definition of objectivity that I have made, means that we are going to learn a terrible lot; there are going to be many different ways of talking about things; the range from almost un-understood practice to recondite and abstract thought is going to be enormous. It means there are going to have to be a lot of psychologists, as there are getting to be a lot of physicists. When we work alone trying to get something straight it is right that we be lonely; and I think in the really decisive thoughts that advance a science loneliness is an essential part. When we are trying to do something practical it is nice to have an excess of talent, to have more sailors than are needed to sail the ship and more cooks than are needed to cook the meal; the reason is that in this way a certain elegance, a certain proper weighing of alternatives, guides the execution of the practical task.

We are, for all kinds of reasons, worrying about how our scientific community is to be nourished and enough people who are good enough are to come and work with us. And then on the other side we are worried about how we are to continue to understand one another, and not get totally frustrated by the complexity and immensity of our enterprises.

I think there are good reasons of an inherent kind, beside the competitive compulsion of the communist world, why we would do well to have more and better scientists. I know that exhortation, money, patronage, will do something about this; but I do not think that is all that will be needed. I think that if we are to have some success it must be because, as a part of our culture, the understanding, the life of the mind, the life of science, in itself, as an end as well as a means, is appreciated, is enjoyed, and is cherished. I think that has to be a very much wider thing in the community as a whole, if we are to enjoy with the community as a whole the healthy relations without which the developing powers of scientific understanding, prediction, and control are really monstrous things.

It may not be so simple, to have in the community at large some genuine experience of the pleasures of understanding and discovery. It may not be simple because what this requires is not merely that this experience be agreeable, but that it have a touch of virtue; that not only the consideration of ends, of products, of accomplishments and status, but the texture of life itself, its momentary beauty and its nobility, be worth some attention; and that among the things that contribute to these be the life of the mind and the life of science. Let us try to make it so.

# FORMALIZED PSYCHOLOGICAL SERVICES IN STATE EDUCATION PROGRAMS

T. ERNEST NEWLAND

*College of Education, University of Illinois*

SCHOOL psychologists receive formal recognition in state school codes, state department or state board of education regulations, or in the regulations governing the operation of their local school districts. In some instances, local provisions have preceded the establishment of regulations at the state level; more commonly in recent years, the state regulations have played a major part in determining the nature of local school requirements. Local district regulations tend to be no less than those of the state, and often exceed them.

Both the numbers of states requiring certification of school psychologists (or those presumed to be serving in such a capacity) and the nature of such certification requirements have been reported in the literature. Horrocks (2) reported in 1946 on the regulations of seven states and Claytor (1) reported in 1950 on those of nine states. Studies likewise have been made by the Division of School Psychologists and have been reported in the newsletters of the division. The studies have differed considerably in their execution, precluding any general synthesis of their findings. A more intensive and recent study of the certification problem at the state level seemed indicated in order to provide part of the background information for the Thayer Conference on the functions and training of school psychologists.

The information summarized here has been obtained directly from copies of school laws, from copies of state department regulations, and from correspondence with state department officials. This summary incorporates only provisions that have been enacted into law and/or have been promulgated as regulations by the state departments of education in bulletin form. Understandably, information supplied regarding aspirations, plans, and even proposed legislation, interesting and promising as they may have been, could not be incorporated into a description of the *status quo*.

A preliminary form of this summary was prepared and a copy of it was sent to each chief state education official, and to the superintendent of schools in the District of Columbia, with an explanation of its purpose. Each was invited to read it critically and to indicate any corrections that should be made with respect to his school program. Corrections and additions have been made on the basis of information received up to November 15, 1954.[1]

The regulations summarized here have tended to be the ones applicable to or growing out of state special education programs, since this has been the principal avenue through which formalized psychological services as such have been introduced into the schools. This delimitation of the survey is not taken as indicative of any insensitivity to the fact that psychological services are provided in and to the public schools in connection with guidance programs, through visiting counselors who work essentially as teachers or as social workers with emotionally maladjusted children. Nor does it indicate an unawareness of the contributions of community mental health clinics, state departments of health, welfare, or mental hygiene, and by college and university psycho-educational clinics.

The term *department* will be used to designate the state education agency, regardless of its local title. When the name of the state is used, reference always is made to the department. The term *psychologist* is used here in a very broad sense and, in many instances, at variance with the usage in the particular state being reported on. At the end of this report is a list of the various terms by which those rendering this kind of formalized psychological service in the schools under these legal provisions have been designated.

It is well to keep in mind the fact that two levels of formal recognition of psychological service are involved. On the one hand, there may be specific statements in the school laws of requirements, functions, or designation of psychological personnel. Such designations may appear also in formal statements issued by the departments, usually (but not always) on the basis of specific legal authorization

[1] The dates of the officially published sources of the information used in this report were: *1954*—Conn., Neb., N. C., Ore., S. C., Va.; *1953*—Colo., D. C., Ga., Ill., Iowa, Ky., Minn., Mo., Mont., Nev., N. H., N. J., N. M., N. D., Okla., Pa., R. I., S. Dak., Tenn., Utah, Vt., W. Va.; *1952*—Ariz., Cal., Fla., Miss., Ohio, Tex.; *1951*—Ala., Del., Idaho, Ind., Kan., Md., N. Y., Wash., Wis., Wyo.; *1950*—La., Mass., Mich.; *1949*—Ark.; and *1945*—Me.

for such statements. For instance, a school law may provide that a child believed to have a given characteristic should be examined by a psychologist (or specialist) qualified according to the standards or regulations established by a state board of education. The board then determines those qualifications, and the department promulgates and enforces both the law and the board regulations.

*To What Extent Do States Have Formally Legislated Provisions for Psychological Services?*

Psychologists are mentioned specifically in the school laws of thirteen states: Arkansas, California, Colorado, Connecticut, Illinois, Mississippi, Nebraska, New Mexico, New York, North Dakota, Pennsylvania, South Dakota, and West Virginia. In nine other states the law is worded in such a way as very plausibly to imply that psychological service is to be rendered by a person regarded as falling among "other experts" or "other specialists." These states are Florida, Georgia, Indiana, Kansas, Louisiana, Maryland, Missouri, Ohio, and South Carolina.

*To What Extent Do States Formally Recognize the Qualifications of These Psychologists?*

Six states indicate in a wording of the school law that psychologists are to be certificated or have a similar form of approval by the department: Arkansas, California, Colorado, Illinois, Mississippi, and Pennsylvania. In fourteen other states, and in the District of Columbia, the certifiability of psychologists is provided for in the regulations of the departments. These states are: Arizona, Connecticut, Delaware, Florida, Indiana, Iowa, Michigan, Missouri, New Jersey, New York, Ohio, Oregon (regulations adopted effective 1956–57), South Carolina, and Wisconsin. While the Nebraska school laws provide that handicapped children shall be examined by an "educational psychologist," the only stated requirement for this "qualified psychologist" is that he "hold membership in the American Psychological Association."

The "counselors" who are provided for specifically in Texas' school law, and whom the department would classify as psychologists are essentially guidance oriented. In Arkansas and Kansas, the certification of the psychologist is required by law, but no standards appear to have been established. In Louisiana and Wyoming, other departmental regulations ostensibly require certification but the standards appear not to have been established. Arizona has a generalized "certificate of specialized service" which apparently provides for a type of psychological certification at one level. A master's degree is required, or "30 hours of graduate work acceptable toward an advanced degree and acceptable to the State Board of Education." An undergraduate major in the relevant area is required. Maryland certifies psychologists, presumably at one level, under a comprehensive certificate requirement for directors and supervisors. In some states, recommendations have been made that some kind of psychologist be used in the examination of children, but no standards have been established. Illustrative of this situation are North Carolina, Virginia, and Washington. In a few other states, like Wyoming, the universities and colleges provide approval of persons to make psychological examinations for the schools. While not provided for specifically in law, Georgia and Vermont are seeking state department level psychologists with at least master's degrees in clinical psychology. Vermont is requiring at least one year's school teaching experience with another year's experience in child guidance work or in special education. These persons will be expected to render at least part-time psychological services directly to the public schools. The Virginia department, as of July 1954, "insists" that children enrolled in special classes for the retarded be examined by persons certified by the State Board of Examiners as clinical psychologists.

*What Amounts of Professional Training Are Required for the Different Levels of Persons Rendering Psychological Service in the Schools?*

Indiana provides for three levels of certification. Six states, California, Connecticut, Delaware, Michigan, Ohio, and Pennsylvania, provide for two levels of psychologists. The District of Columbia and these seventeen states have one level of certification or less formal basis of recognition: Arizona, Colorado, Florida, Georgia (only in the state department), Illinois, Iowa, Kansas (implied but not stated), Maryland, Mississippi, Missouri, New Jersey, New Mexico, New York, Oregon, South Carolina, Virginia, and Wisconsin.

Mississippi's newly legislated program requires a doctoral degree of its psychologist. Indiana's highest level psychologist is required to hold a doctoral degree. For its higher level of certification, Connecticut requires a doctoral degree for the standard certificate, although provisional certification as a

school psychologist is possible with at least 60 hours on a doctoral program.

Fourteen states specify the master's degree as one of the requirements for certification at at least one of their levels, thirteen of these requiring a major in psychology or a master's degree in psychology. Along with Oregon's requirement (effective 1956–57) of a master's degree, 64 hours "taken prior to or subsequent to" the bachelor's degree are required. South Carolina provides that the psychologist must have a master's degree in psychology when he is not "a recognized clinical psychologist, school psychologist, or private consulting psychologist previously accepted by physicians, social agencies, or school systems for his competence."

Twelve states specify the bachelor's degree, or the bachelor's degree plus some number of credit hours, among the requirements for at least one level of certification. Only six of these states require an undergraduate major in psychology.

The fact that some states require only a bachelor's degree may be somewhat confusing since the additional hours they require along with the bachelor's degree may represent actually the possession of a higher degree. Pennsylvania, for instance, requires a bachelor's degree (with the major not specified) plus 66 hours[2] of course work in the psycho-educational field for its higher level of certification. Michigan requires a bachelor's degree and 36 hours (at least 24 of which must be graduate) for "temporary" certification as a diagnostician. In like fashion, Alabama requires the bachelor's degree, with a major in psychology, and 30 hours' additional work. Florida does not specify the undergraduate work, but requires 60 hours of work beyond the bachelor's degree. Maryland requires an additional year beyond a bachelor's degree with a major in psychology in the graduate year. New York requires 30 hours, and Wisconsin 24 in addition to the bachelor's degree. The higher certification level in California requires a master's degree with a major in psychology, a specified 36 hours in psychology having been taken at the graduate and/or undergraduate levels. In Ohio, the only difference between the junior psychologist certification and the full certification is that an individual must have given 24 months of satisfactory service at the junior level in order to qualify for the senior level. Indiana's intermediate level of certification requires a bachelor's degree with a major in psychology. Its lowest level requires only a bachelor's degree with a major in psychology (at least 11 hours in the psycho-educational area in connection with the bachelor's degree).

The extent to which clinical practice is included in such statements of hours' credit varies considerably. Some states include definite provisions for the inclusion of a given amount of such practice—from eight per cent up to as much as 40 per cent. In Pennsylvania, a minimum of 135 clock hours of clinical practice (three credit hours) will suffice for this aspect of lower level certification, while some 1,100 hours of such work may be allowed toward the higher certification. In other states, amounts of clinical practice may be substituted for specified numbers of required credit hours. In Oregon, for instance, two years of properly supervised clinical experience can be substituted for some 12 hours' credit for clinical practice.

*To What Extent is Supervised Practicum (or Internship) Experience Required OTHER THAN THAT Which is Reflected in the Formal Hour Requirements?*

In seven states such practicum experience, over and above the credit requirements, is specifically designated. In the case of Arizona, the equivalent of eight credit hours is expected. Colorado requires 100 clock hours of such clinical practice; New Jersey, 150 clock hours; Ohio, 300; Indiana, 350; Illinois, two years of full-time experience, not less than 25 hours per week; and Michigan requires two years of experience, one of them to be of internship and/or clinical experience with children.

*To What Extent Are School Psychologists Expected to Hold Certification as Teachers?*

In thirteen instances teacher certification is specifically required. These are: California, Colorado, the District of Columbia, Florida, Indiana (the middle grade of certification), Iowa, Michigan (or membership in the Michigan Psychological Association or in the American Psychological Association), Missouri, New Mexico, Ohio, Oregon, Pennsylvania (for the County Supervisor of Special Education, not for the Public School Psychologist or the Public School Psychological Examiner), and Wisconsin. In the case of Wisconsin, the person must have also two years of school testing experience. Teacher certification appears optional in the case of California's lower level certification—teacher certifica-

[2] The term *hours* is used here only in the sense of semester hours, transmuted where requirements were stated in terms of quarter hours, and assumed where the requirements did not indicate which was intended.

tion "or bachelor's degree." One slightly different aspect of this situation occurs with respect to Indiana's highest level of certification, in which case the psychologist is required to hold certification as an administrator or supervisor and no mention is made of a teacher's certificate per se, unless it is assumed to precede certification as administrator or supervisor.

*What Provisions Exist with Respect to the Renewal of Certification of Psychologists?*

In at least ten instances, specific provisions are made for the renewal of such certification. In the case of Alabama, the initial certificate is issued for four years and may be renewed for six years on evidence of two years' satisfactory experience and five additional hours of professional training. The "specialized certificate" of Arizona is issued initially for four years and is renewable for a period of six years either after two years of such specialized work plus five hours' credit for acceptable work or on evidence of continuous specialized work for four years. In California, the lower level certificate is issued for two years and may be renewed for five-year periods. Its higher level credential is issued for a period of time which corresponds to the term of the individual's basic teaching credential, except that, in the cases of those holding life credentials, the psychological credential is issued for five-year periods and is renewable for similar periods. Connecticut issues provisional certificates for three years at both levels. These may be converted into "standard certificates" after three years of successful experience under the provisional certification. Iowa's initial certificate is issued for five years and can be renewed for a five-year term after the individual has given evidence of eight months of satisfactory service and having taken six hours of professional training. This latter can be renewed at expiration for life after the individual has had a total of five years of satisfactory service, provided two of them occurred during the most recent five-year term of this certificate. In Maryland, the initial certificate is issued for three years, may be renewed for four, and after that may be renewed for periods of six years each. The certificate in Missouri can be made permanent after "three years of experience in school psychological work." The New Jersey certificate is issued initially for three years and may be converted at the end of that time, assuming successful experience, into life certification. In New York, the initial certificate is issued for five years and may be converted to life certification by presenting evidence of having taken 12 additional hours of training. After life certification has been obtained, it can be held by the person's showing evidence of having taken six hours of professional work each ten-year period subsequent to the issuing of the life certificate. Ohio's initial certificate is issued for four years but may be renewed at the end of 24 months' satisfactory service in the field and converted into an eight-year certificate. After 40 months of satisfactory service, it can be made permanent. In Oregon, after five months' successful experience obtained on the basis of the initial one-year certificate, a five-year certificate can be issued. This latter can be renewed for five years after 25 months of successful experience.

*What Other Kinds of Psychological Service Are Provided or Used?*

In three states, Georgia, Tennessee, and Vermont, a psychological service is provided directly to the schools out of the department, usually by the Special Education staff. Vermont reports the supplementary use of state mental health clinics.

The departments in eight states give no evidence of providing psychological service either directly (out of the state office) or indirectly (through the setting up of state requirements or other form of encouragement). Arkansas gets its psychological service through "various state colleges." Maine relies upon 12 community clinics operated by the State Division of Mental Health, while Massachusetts utilizes services provided by the State Department of Mental Health. In Minnesota, the State Bureau of Psychological Services provides this type of service systematically and formally for the department. The Montana mental hygiene clinics under the State Board of Health provide psychological service for the schools. New Hampshire's Child Guidance Center and Mental Hygiene Clinic makes the psychological evaluations for the department. North Carolina utilizes psychological services provided by the State Board of Public Welfare and by mental health clinics. North Dakota obtains its psychological services only outside the formal school program.

Numerous states use directly, or encourage their local school districts to make use of, mental health, health, and children's bureau clinic facilities, either as an adjunct to their formal plan for psychological services or as a substitution for their lack of such a program.

*To What Extent Does There Appear to Be No Indicated Sensitivity to a Need for Such Psychological Services?*

On the basis of material made available for this study, it would appear that only a small number of departments feel no special need for psychological services in their schools. Idaho, for instance, assumes no responsibility with respect to the psychological examination of its children, referring those who may be concerned with such problems to persons who have been licensed by the State Occupational License Bureau which reportedly licenses psychologists for the examination of children. The point was made rather specifically, however, that this is not in connection with the public school program. According to a December, 1953 letter, "For the most part, Kentucky does not have psychological evaluation for children in special education services." Whether or not this reflects no felt need in this area may be a matter of interpretation. Nevada and Rhode Island similarly reflect no felt need for such psychological services.[3]

*By What Titles are Psychologists Designated in School Law and/or Official Department Statements?*

*School Psychologist*—Florida, Iowa, New Jersey, New York, and Tennessee.
*Psychologist*—Arkansas, Colorado, Georgia, Louisiana, Minnesota, Mississippi, and Missouri.
*School Psychologist* and *Psychological Examiner*—Connecticut and Delaware.
*School Psychologist* and *School Psychometrist*—California.
*Qualified Psychological Examiner* (in the law), plus, elsewhere: *Staff Psychologist, Area Psychologist, Area Chief Psychologist,* and *Clinical Assistant*—Illinois.
*School Psychologist, School Psychometrist,* and *School Testing Technician*—Indiana.
*School Psychologist* and *Junior School Psychologist*—Ohio.
*Public School Psychologist, Public School Psychological Examiner,* and *County Supervisor of Special Education*—Pennsylvania.
*School Psychometrist*—Wisconsin.
*Psychological Examiner*—Missouri and South Carolina.
*Diagnostician*—Michigan.
*Examiner*—Kansas.
*Clinical Psychologist*—South Dakota.
*Psychological Tester*—Wyoming.
*Educational Psychologist*—Nebraska.
*Counselor, Director of Guidance*—New Mexico.
*Research Assistant*—District of Columbia.

[3] In Texas, the use of group intelligence tests is suggested for determining eligibility for placement in special classes for the mentally retarded, "but if there is the slightest doubt" as to the accuracy of the results so obtained, a "competent psychologist" should make an individual examination of the child in question (Texas Bulletin, 1952). In Utah, a 1951 school law provided for the examination of handicapped children by a medical person, or a State Board of Education approved "public school psychologist or psychological examiner." In 1953 a school law, presumably amending the 1951 law, places the responsibility in the hands of a "competent physician."

Numerically and probably also qualitatively, the legal recognition of school psychologists has improved somewhat over the conditions reported in 1946 and 1950. They are mentioned specifically in the school laws of thirteen states and may be regarded (at least by psychologists) as alluded to in the school codes of nine other states. Their certification is provided for specifically in the school laws of six states and officially incorporated in the state department regulations of an additional fourteen states and the District of Columbia. From one to three levels of certification are provided. Minimal degree requirements vary from the doctor's to the bachelor's, the latter heavily predominating. Numbers of credit hours tend to be specified more than do degrees. The amount of practicum experience, when it is specifically required in the cases of seven states, ranges from 100 clock hours to two years of experience. Certification as a teacher is specified in thirteen instances. Specific renewal provisions are established in ten instances, the amount of preceding work experience being highly variable. In eight states, psychological services are provided the public schools by other than education agencies, the patterns varying from systematically planned, state-wide programs to the sporadic use of such facilities. Major or complete insensitivity to a need for psychological services is suggested in only three states. School psychologists function under at least sixteen different legally established titles.

It is hoped that the outcomes of the deliberations of the Thayer Conference (published by the APA under the title *School Psychologists at Mid-Century*) will contribute, in the not too distant future, to a simplification and clarification of the legal status, qualifications, designations, and functions of those rendering this type of psychological service in the public schools.

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# THE BIO-SCIENCES INFORMATION EXCHANGE OF THE SMITHSONIAN INSTITUTION: A CLEARINGHOUSE FOR RESEARCH IN THE BIO-SCIENCES [1]

STELLA LECHE DEIGNAN [2]

*Bio-Sciences Information Exchange*

THE Bio-Sciences Information Exchange is a development from the Medical Sciences Information Exchange, of the National Research Council, enlarged and renamed in recognition of its expansion into biological and psychological fields. Its increasing use as a clearinghouse for current research vividly demonstrates the value of an exchange of information on work just beginning or not yet ready for publication. This paper is designed to introduce the Exchange to those members of the APA who are not acquainted with it, to stimulate registration of additional research, and to augment the use of its services.

The history and development of the Exchange have been previously described (1, 4); it will suffice here to outline briefly its present status and to discuss those functions of special interest to investigators.

Although the Exchange derives its total support from the seven government agencies which award grants or contracts for research in the life sciences, the cooperation of the major fund-raising agencies and private foundations has been a valuable asset and a source of much of the material in the Exchange. These national granting agencies, both government and nongovernment, inform the Exchange of their awards. The Exchange then requests the recipients of awards to provide brief summaries of their research problems for the purposes of exchange of information. The request carries a full explanation of the uses and dissemination of the material.

How much of the nation's research is supported from extramural sources is not known. It is evident that many investigations are carried on without benefit of additional funds, and that a clearinghouse which included intramural research would be more complete. In this connection the Exchange is meeting with some success in that a number of universities have begun to participate. Actually, however, the majority of the approximately 7,000 active research projects presently registered are supported, at least in part, by national granting agencies.

What useful purpose is served by the clearinghouse? Its primary use is to coordinate the information from all sources and to keep the supporting and cooperating agencies currently informed on (*a*) the amounts and sources of support of investigators, institutions and geographical areas, and (*b*) the amounts and sources of support of broad and specific subject fields. It is in carrying out this second responsibility that the Exchange has become a clearing house which serves not only the granting agencies, but investigators and research institutions as well.

The development of a detailed subject index and the assemblage of professional staff with competencies in the biological, psychological, and medical fields has enabled the Exchange to put investigators in contact with others having similar interests by the simple device of providing copies of germane summary statements. The value of the Exchange obviously rests upon the quality of the summary material registered with it, the exactitude of the indexing, and the background knowledge and intelligence exercised in selecting pertinent material. The quality of the summaries is constantly improving, and, if letters of appreciation from users of the Exchange are a measure, the service is an effective one.

The fear, by some, that investigators might refuse to write summaries of their research plans because of an objection to the preparation of "another piece of paper" or because of an apprehension that their ideas might not be protected from premature pub-

[1] This article appeared in *The A.I.B.S. Bulletin* for October 1954. It has been adapted for the *American Psychologist* by Betty R. Horenstein, Professional Associate, Psychological Sciences, Bio-Sciences Information Exchange.

[2] The views and opinions expressed here are those of the author and not necessarily those of the members of the Governing Board of the BSIE or the agencies they represent.

lication, has proved to be unfounded. Investigators are usually very cooperative in providing the Exchange with adequate summaries of their proposed or current research. They are also protected from premature publication of their work by a clear understanding, indicated on the form containing the summary and explicitly repeated in the reply to every request for information, that the material sent out is for the use of the recipient alone and not for reference in any publication without special permission directly from the persons furnishing the summaries. Besides, the writer of a summary is requested to omit from it any information that he considers confidential. There has been no instance of misuse of material sent in response to requests. Of the more than 5,000 principal investigators and 7,000 other professional persons associated with the research comprising the body of the information in the Exchange, only five have raised any questions on this point. One of these felt so strongly on reporting of any kind, even to agencies supporting his laboratory, that he wrote a paper on the subject and sent a reprint of it in response to the request for a summary. Another changed his attitude after he had had occasion to use subject information from the Exchange and has become a mild enthusiast. On the other hand, some investigators have even requested the Exchange to include their research in its roster.

Experience has shown that the best service is rendered through the technique of specific requests. The practice of periodic distribution of information was discarded after a trial period because the volume of material, even after division into broad subject fields, was overpowering and burdensome to the recipients. Under the present system, the screening and selection of material used in response to specific requests is done by the staff of the Exchange.

Requests for subject information come from three general sources: granting agencies, committees, and individual investigators. The kind of information made available to the first two groups differs in certain aspects from that which may be supplied the individual investigator. Except for the type of information carried in published reports (2, 3), only cooperating granting agencies are privileged to obtain information on amounts (dollars). It would be impractical to perform detailed surveys of all work in comprehensive fields for individuals, whereas it is reasonable to prepare them for properly constituted committees. The service to individual investigators is consequently limited to work related to one or a series of special problems.

By way of illustration, a new granting agency, or an established one considering a reorientation of its program, may receive a detailed survey of the extent of research and amounts of support available for work in a broad field and showing the emphasis of research support in the various subcategories into which the field may be divided. New granting agencies use this service to determine areas most in need of support. Similar surveys are performed for committees charged with the consideration or stimulation of special fields of research. The service to investigators, on the other hand, provides a knowledge of who is working on a specific problem or approach to a problem. In providing this service the Exchange differs from a reference library, in that it does not refer an investigator to a publication, but enables him to contact directly those with immediate and similar interests. The more specifically a request for information is phrased, the more efficient can be the service rendered in complying with it. The index maintained by the Exchange is so constructed that the professional staff can extract with considerable precision the particular work in which the requester is interested, thus saving him from having to read large numbers of summaries in which his field of interest is referred to only as a remote parameter of the other types of research. Requests for research in broad fields, such as "all" work on "clinical psychology" or "mental health" would embrace such a volume of research that the Exchange (unless the request came from a committee) would reply by sending a sampling of projects, whereas a request for "development of criteria for evaluating changes during and after individual psychotherapy" is sufficiently explicit to enable the staff to provide all pertinent research. Similarly, again with the exception of committees, a request for "research in physiological psychology" covers too broad and varied a field for detailed reply, whereas one for "the psychological effects of radiation" adequately defines the particular interest of the investigator.

A recent review of approximately one hundred requests from investigators confirmed the belief that no two requests are alike. Most research is marked by an individuality in approach or in frame of reference and this valuable characteristic is pre-

served in the systems of the Exchange. The index has proved a most effective tool, whether in the grouping of related research or in locating particular problems. It permits the staff to organize the material in a variety of ways and to find, with a minimum of effort, research bearing on a special problem.

It has often been suggested that the index be published or be made available for distribution in

NOTICE OF RESEARCH PROJECT
BIO-SCIENCES INFORMATION EXCHANGE
SMITHSONIAN INSTITUTION

NOT FOR PUBLICATION OR PUBLICATION REFERENCE

ACCESSION NO. (Do not use this space) XYZ-5678

NAME AND ADDRESS OF INSTITUTION:

University of , Address:

TITLE OF RESEARCH PROBLEM.

Learning, Retention and Recovery of Meaningful Material

Give names, departments, and official titles of ALL PROFESSIONAL PERSONNEL engaged in the research.

_____. Professor of Psychology

_____. Graduate Assistant

SUMMARY OF RESEARCH PROBLEM— (200 words or less — Omit classified data and other confidential information.)

In the Bio-Sciences Information Exchange summaries of work in progress are exchanged with government and private agencies supporting research in the bio-sciences and are forwarded to investigators who request such information. Your summary is to be used for these purposes.

Experiments are to be designed and carried out to determine how verbal organizing factors influence and affect the learning and retention of meaningful material. This will require theoretical development and specification of Bartlett's concept of the schema. Some of the variables which will be involved in the experiments are characteristics of the material, subject variables, length and characteristics of the retention interval.

Experiments will also be conducted on a related problem, recovery of material from memory without consultation or relearning of original sources. It is postulated that in the act of attempting to recall lost material and in repeatedly attempting to do so, any improvement in recall may be due to the ability to reinstate associated cues and to the spread of reinforcement to less available aspects of an organized memory system. The assumption is made that recalling part of the material raises the availability not only of what is recalled but of what is related to it. Variables to be studied are characteristics of the material, cues at the time of recall and postulated characteristics of a mediational process.

SIGNATURE OF PRINCIPAL INVESTIGATOR Graduate School DATE

INVESTIGATOR—DO NOT USE THIS SPACE

FIG. 1. Sample: "Notice of Research Project" form.

some other manner. The index was developed for a special purpose and its usefulness beyond this purpose is doubtful. It changes daily in accordance with the appearance of new research and it frequently undergoes major and minor rearrangements to conform with new concepts or points of view. Its structure is not that of a classification nor that of an ordinary index; it is, rather, a series of independent indexes, each of which is complete for the topic under consideration. When a new interest or a different point of view develops, a new section of the index may be added with a minimum of effort. This structure is necessary because of the responsibility of the Exchange to correlate subject matter with the number of awards and amounts granted, and to reflect, on a daily basis, the myriad of administrative details coincident with the administration of programs supporting research through grants and contracts. These details dictate, in part, the structure of the index and limit its usefulness for other purposes. Its effectiveness within the Exchange is unquestionable, as is demonstrated by the rapidity with which subject requests are answered. A statistical analysis showed that the mode of the time required to answer all subject requests, including the survey type, is three working days, and 44 per cent of the requests are answered in one, two, and three working days.

A growing body of information on the interests and policies of national granting agencies, gathered in the normal course of its activities, permits the Exchange to assist investigators seeking sources of support. Exploration of purely local foundations soon led to the conclusion that it would not be practical to include their activities in the Exchange. The following statement from a recent report (3) partially explains the problem: "Considerable thought has been given to the wisdom of gathering concise information from purely local foundations. The diversity of such organizations and the lack of established granting policies with regard to type of research supported or the probable duration of interest in any research field have reasoned agains the inclusion of these data. Most local foundations appear to be mechanisms for family or individual giving with primary interest in community services and welfare."

It is the exceptional local foundation which employs a permanent staff or which formulates or follows a consistent policy. The names, and sometimes the addresses, of the many local foundations which reach the Exchange have been listed by state in the belief that such lists may assist research institutions in their search for local sources of funds.

Investigators whose work is represented in the BSIE may be invited to participate in symposia, conferences, or cooperative research programs. They may even receive unsolicited additional support on problems of special interest to certain industries. These are side issues resulting from the increasing use of the Exchange as a clearinghouse.

The standard form on which research is registered with the Exchange is familiar to all recipients of awards from national granting agencies. This "Notice of Research Project" form was designed by representatives of the supporting agencies when the Exchange was established and has required only slight modification in the subsequent years. There are two variations of the form, one for extramurally supported research and the other for intramural research. They differ only in that the one used for grant- or contract-supported research has a space for the name of the granting agency. The forms are supplied, free of charge, to investigators who wish to record their research with the Exchange.

The summary contained in Figure 1 is reproduced with the permission of the principal investigator who submitted it. While it contains neither results nor report of progress, it illustrates, by its completeness and simplicity, the kind of abstract which effectively outlines a research problem in its initial stages. Brief summaries comparable to this are becoming recognized as a medium for the rapid and free interchange of scientific information. Such an exchange will be increasingly valuable as investigators become accustomed to employing this means of communication for work which has not reached the publication stage.

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# PARTICIPATION IN THE APA

CHESTER C. BENNETT AND JOYCE B. KAYE

*Boston University*

THE APA Committee on Participation periodically filters the membership for new blood. They have made specific inquiries about psychologists active in state association affairs whose talents have not found expression in the national association. Such an inquiry stimulated our curiosity and suggested an empirical analysis of APA participation habits. We are not familiar with any soundly based participation theory. It is a popular assumption that participation in organizational affairs is good for the organization and/or the participants, and that much participation by many is better than a little by a few. A developed theory might have to reckon with complex interactions between concentration of power and diffusion of effort, good intentions and inertia, tested experience and new blood.

The organizational structure of the APA is reflected in the annual publication in the November *American Psychologist* of a roster of officers, editors, boards, and other functional groups. The observations which follow are based on an analysis of these rosters for 1951 and 1954 (1, 2). The investigation of two rosters was prompted by an interest in time trends, and the three-year interval was chosen to bracket the three-year tenure governing many offices and committee assignments. The published information was transcribed to individual name cards. Our attention was focused on the number of APA members involved, the concentration of responsibilities delegated to a few, and tendencies to recidivism. The rosters include officers of state associations and this group will figure in the discussion. The omission of committee work of Divisions, CSPA, regional, and state associations should be recognized as limiting the participation picture here reported.

Table 1 provides an overview of the APA structure and suggests the dimensions of our information. The categories in general conform to the headings employed in the *American Psychologist*. Identified with each category is the number of specific assignments or functions reported. We will call them "duties," with apologies to those who would emphasize the honorific implications of participation. It is obvious that a duty is not a standard unit. Individuals may have more than one duty —representatives to the Council, for example, are also listed automatically as officers or Division Executive Committee members. The nomination of Past Presidents to the American Psychological Foundation is automatic, and there are ten or twelve other duties which are either explicitly or implicitly *ex officio*.

The table shows a total of over 500 duties, increased by 12 per cent over the three-year period. There is a stable nuclear organization of elective officers, and standing committees. Increased participation appears principally in special committee work. The Education and Training Board, together with its subcommittees, represents about 25 per cent of special committee duties. A concentrated effort was focused on the Committee on Ethical Standards in 1951. Other special committees have multiplied. There were seventeen in 1951, of which twelve were still active in 1954. The emergence of twelve new committees and boards brings the 1954 total to twenty-four, and ABPS and APF were also new. We note an increase in representation in other or-

TABLE 1

APA Offices, Boards, and Committee Assignments

| Category | Duties 1951 | Duties 1954 | Per Cent Increase |
|---|---|---|---|
| Officers | 6 | 6 | |
| Board of Directors | 6 | 6 | |
| Council of Representatives | 68 | 67 | |
| Divisions and CSPA | | | |
| Officers and Executive Committees | 159 | 169 | 6.3 |
| Editors | 11 | 11 | |
| Standing Committees | 52 | 50 | |
| Special Committees: | | | |
| Education and Training Board | 46 | 47 | |
| Committee on Ethical Standards | 40 | 7 | |
| Other Special Committees | 82 | 123 | 50.0 |
| Representatives to other Organizations | 37 | 43 | 16.2 |
| Regional Association Officers | | 22 | |
| ABEPP | | 9 | |
| ABPS | | 5 | |
| APF | | 9 | |
| Totals | 507 | 574 | 12.0 |

ganizations. The increase represented by regional association officers and ABEPP is an artifact, since these groups were active but not listed in 1951.

Table 2 represents people. APA membership figures are based respectively on the 1951 Directory, and the 1953 Directory corrected for resignations, necrology, and elections to membership reported in the *American Psychologist*. The Association has grown by 40 per cent in three years. The number of participants has increased approximately 16 per cent. The number of people listed as state association officers shows an increase of 45 per cent, more commensurate with the growth of the Association. The final category of Table 2 shows the unduplicated names appearing in the two rosters. There were nineteen people in 1951 active in both State and APA affairs, and twenty-five people so listed in 1954. It is noteworthy that a participation increase of 12 per cent expressed in duties is associated with an increase of almost 16 per cent in the number of people involved. Statisticians will find this difference significant at the 7 per cent level.

TABLE 2

PARTICIPATION OVERVIEW

| Category | *N* 1951 | *N* 1954 | Per Cent Increase |
|---|---|---|---|
| APA Membership | 8,554 | 12,097 | 41.4 |
| APA Participants | 273 | 316 | 15.8 |
| State Association Participants | 119 | 173 | 45.4 |
| Total Participants | 373 | 464 | 24.4 |

Analysis of individual participation showed one name appearing nine times in each roster. Guess who. Apparently our Executive Secretary was working as hard for 8,500 members in 1951 as he did for 12,000 in 1954.[1] With nine as a maximum, the assignment of multiple duties tends to concentrate in Council representatives. An occasional non-Council member is listed as many as four times. It is noteworthy, however, that in each roster over half the names reported appear only once.

The concentration of participation is summarized in Table 3. A literal reading states that the Council, comprising less than 1 per cent of the membership, was responsible for 45 per cent of the duties in 1951, 38 per cent in 1954. Only one in a hundred of the membership had more than one duty assignment in 1954, and their names represented two-thirds of the roster. In summary, the table suggests that participation in APA affairs is confined to a decreasing *proportion* of the membership, now less than 3 per cent. To show the versatility of statistics, we can also observe, from Table 2, that participation involves an increasing *number* of members, now totaling over 300 people. Neither statement does justice to the true participation picture, which should include division committees, subcommittees, and *ad hoc* consultants, plus a good deal of the Executive Secretary's incoming mail—participation without portfolio.

TABLE 3

CONCENTRATION OF PARTICIPATION

| Category | 1951 | | 1954 | |
|---|---|---|---|---|
| | Per Cent of Membership | Per Cent of Duties | Per Cent of Membership | Per Cent of Duties |
| Council | .8 | 45.9 | .6 | 38.2 |
| 3 or More Duties | .7 | 45.9 | .6 | 45.5 |
| 2 or More Duties | 1.4 | 70.6 | 1.0 | 68.1 |
| All Participants | 3.2 | 100.0 | 2.6 | 100.0 |

In Table 4 we see a partial answer to the hoary question whether the same "old guard" is running the APA year after year. This table shows a breakdown of 1954 participants in relation to their 1951 participation status. Of 68 members of the 1951 Council, seven were still on the Council in 1954, thirty-seven held other APA duties, and twenty-four did not appear in the roster. This suggests a three-year turnover of 90 per cent for Council membership. Of the total 273 APA participants in 1951, 146 or 53 per cent were still active in 1954 and constituted 46 per cent of the 1954 total of 316. The three-year turnover for total participation is roughly 50 per cent.

Table 4 also sheds some light on the trigger question, to what extent APA participation is nourished by experience gained in state association affairs.

TABLE 4

PARTICIPATION TRENDS

| 1954 Status | 1951 Status | | | | 1954 Totals |
|---|---|---|---|---|---|
| | Council | Non-Council | State Only | Unlisted | |
| Council | 7 | 33 | 3 | 24 | 67 |
| Non-Council | 37 | 69 | 9 | 134 | 249 |
| State Only | 0 | 3 | 23 | 122 | 148 |
| Unlisted | 24 | 100 | 65 | (11,444) | 189 |
| 1951 Totals | 68 | 205 | 100 | 280 | 653 |

[1] There were just as many hours in 1951 as there were in 1954.—Ed.

Again, the findings are biased by an incomplete picture of state association committee work. Of 100 state association officers listed in 1951 and not already participating in APA duties, only twelve appear among the 1954 participants. Three of these had achieved Council membership. A somewhat heavier "flow" from state to national responsibility might have been expected. On the other hand, 158 or exactly 50 per cent of the 1954 participants did not appear at all in the 1951 roster. Another 122 new names emerge in 1954 as state association officers. Involvement in organizational affairs is evidently making determined and meaningful inroads on the reservoir of 11,000-odd psychologists whose interests are served.

Pending the development of participation theory, it would be pretentious to extrapolate an evaluation of the Association's health and welfare from these findings. A comparable analysis of participation in other scientific and professional associations would certainly be instructive and contributory to participation theory.

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# READABILITY OF READERS [1]

WAYNE ANDERSON
*University of Missouri*

A RECENT study has indicated that how much students learn is influenced by the human interest and level of difficulty of the material presented to them (1). A separate study indicates that readability formulas such as the Flesch count are adequate measures of these factors (7).

How difficult and interesting are the materials presented to psychology undergraduates? Recently Ogdon (8) has done a Flesch-count study on eight general psychology texts and found that they were either "fairly difficult" or "difficult" to read and ranged from "mildly interesting" to "dull."

This study is a continuation of Ogdon's in that the books examined are general readers in psychology intended to be used in combination with regular psychology texts. Because of the large number of writers and areas which are represented in these texts, it is felt that they should also offer some indications as to the difficulty and interest of psychological writing in general.

Five readers served as the subject matter for this study. They were: Crow and Crow, *Readings in General Psychology;* Dennis, *Readings in the History of Psychology;* Dennis, *Readings in General Psychology;* Hartley, Birch, and Hartley, *Outside Readings in Psychology;* Skinner, *Readings in Psychology.*

[1] This paper was written for a course in professional problems taught by Dr. Robert Daniel.

An attempt was made to take samples from enough articles that the total mean and standard deviation of the book's readability did not change by the addition of samples from ten more articles. Three samples of 100 words each were taken from each article. Because of the large number of short articles in Skinner's book, there were many from which only one sample was taken. Such a sample was taken from the second paragraph.

A readability score and an interest score were computed for each article. These were then used to arrive at the mean score and standard deviation of scores for each book. The scores within each book were then correlated to show the relationship between readability and interest.

The readability and interest of articles in various subject fields was also studied. These scores were arrived at by combining the data in the three most recent texts: Dennis (*Readings in General Psychology*), Hartley, and Crow.

The results are presented in Tables 1 and 2. As indicated in Table 1 the books are of about the same degree of difficulty, all being difficult according to Flesch's standards. The range of difficulty within a book is, of course, quite wide. The greatest range is in Skinner's book where the readability scores go from 0 (almost impossible to read) to 74 (fairly easy to read). The smallest range is in Dennis' *Readings in the History of Psychology,* 31 (difficult) to 65 (standard).

Some of the most readable work was done by such writers as Köhler, Freud, Munn, Murphy, James McKeen Cattell, and Lashley. The most difficult article (the 0 score) is a translation of one by Lewin. Other writers who are representative of those who are difficult to read are Pavlov, Carmichael, and Cruze.

Most of the articles are mildly interesting to dull, with the range being from 0 (very dull) to 47 (highly interesting). Among the more interesting writers are Freud, Adler, Dashiell, and Koffka. So many of the writers have human interest scores of 0 that it would be unfair to single any out for particular note.

Because an article is readable does not mean that it will also be interesting. This is shown by the low correlations between readability scores and interest scores. The highest *r* is .38 in Dennis' *Readings in General Psychology*.

Table 2 shows the average readability and interest scores within various subject fields. Except for the material on the nervous system and vocational psychology it would appear that the various fields all range around the fairly difficult level and should be comprehended by the average college student. The bottom two subjects have the added impediment of being dull and one would expect that the college student would find them difficult to concentrate on.

TABLE 1

READABILITY AND INTEREST SCORES OF FIVE GENERAL READERS IN PSYCHOLOGY *

| Text | Readability | | Interest | | *r* | Articles Sampled |
|---|---|---|---|---|---|---|
| | Mean | *SD* | Mean | *SD* | | |
| Dennis (General) | 50 | 11 | 13 | 10 | 38 | 30 |
| Hartley | 49 | 11 | 15 | 9 | 23 | 30 |
| Dennis (History) | 49 | 9 | 8 | 7 | 28 | 24 |
| Crow | 48 | 12 | 12 | 10 | 36 | 40 |
| Skinner | 44 | 14 | 10 | 11 | 33 | 70 |

* The lower any score is the less readable and the less interesting.

TABLE 2

FLESCH READABILITY AND INTEREST SCORES OF SUBJECT FIELDS WITHIN PSYCHOLOGY

| Subject | Readability Score | Interest Score |
|---|---|---|
| Thinking | 53 | 14 |
| Motivation | 52 | 18 |
| Perception | 52 | 16 |
| Sensation | 52 | 9 |
| Nature of psychology | 51 | 14 |
| Attention | 50 | 10 |
| Personality | 49 | 15 |
| Learning | 49 | 11 |
| Intelligence | 48 | 18 |
| Social behavior | 48 | 14 |
| Emotion | 47 | 14 |
| Developmental | 46 | 9 |
| Nervous system | 42 | 7 |
| Vocational psychology | 37 | 3 |

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# PRODUCTIVE PSYCHOLOGISTS

HARRY RUJA

*San Diego State College*

WHO are the most productive psychologists? To answer this kind of question, we generally compile bibliographies. Dennis, for example, recently discovered that, judging by number of publications, E. L. Thorndike, Wundt, Hall, James, and Binet are the five most productive psychologists not now living.[1]

But to publish is one thing and to be read and

[1] DENNIS, W. Bibliographies of eminent psychologists. *Amer. Psychologist*, 1954, **9**, 35–36.

cited may be another. Is not a psychologist's eminence measurable not only in terms of number of publications (by him) but also in terms of number of citations (of him)?

Although I have no such measure for the psychologists Dennis lists, I have recently compiled a list of psychologists most frequently cited in three APA journals during the years 1949–1952.[2] For each journal, the five authors cited most often are as follows:

| *Journal* | *Psychologist* | *Number of references to him* |
|---|---|---|
| *J. exp. Psychol.* | Hull | 195 |
| (183 articles) | Spence | 87 |
| | Hovland | 75 |
| | Hilgard | 69 |
| | Miller, Neal E. | 62 |
| *J. appl. Psychol.* | Lawshe | 51 |
| (112 articles) | Strong | 38 |
| | Guilford | 37 |
| | Thurstone | 32 |
| | Flesch | 31 |
| *J. abnorm. soc. Psychol.* | Bruner | 58 |
| | Freud | 44 |
| (110 articles) | Allport, G. W. | 39 |
| | Lewin | 39 |
| | Frenkel-Brunswik | 35 |

I did not record such references to a given psychologist as may have been made in the two journals other than the one in which he was among the five most frequently cited authors. Since the journals were not equal in the number of articles scanned, the psychologists referred to in a sense did not all have equal chances of being cited. Moreover, the ordering may need some modification in relation to the extent to which an author typically publishes alone or with others. For example, of Bruner's 14 publications which the 58 references cite, 12, or 86 per cent, were written in collaboration with others. In contrast, alone Hull wrote 83 per cent (28 out of 35) of the publications to which reference was made 195 times. Freud was sole author of all his publications cited, while Lawshe collaborated with others on more than 70 per cent of the publications cited. Spence collaborated just about as often (42 per cent) as he wrote alone (58 per cent).

[2] I was assisted in this compiling by Sam Johnson of the University of Oregon.

In the light of these considerations, we must acknowledge that under different conditions of tabulation, the list of most frequently cited authors as well as the order in which they are listed would probably be different.

What particular publications of these authors were cited most often? These perhaps had greatest interest and challenge for psychologists during the years in question. Historians seeking to understand the course of psychological research and graduate students wishing to orient themselves to areas of current psychological interest might find a list of such frequently cited publications of value. Here, then, in order of greatest frequency of citation, are the titles of ten publications to which reference was made most frequently in the three journals during the years 1949–1952 inclusive:

HULL, C. L. *Principles of behavior.* New York: Appleton-Century-Crofts, 1943. 105 citations.

STRONG, E. K., JR. *Vocational interests of men and women.* Stanford: Stanford Univer. Press, 1943. 25 citations.

HILGARD, E. R. *Theories of learning.* New York: Appleton-Century-Crofts, 1948. 23 citations.

HILGARD, E. R., & MARQUIS, D. G. *Conditioning and learning.* New York: Appleton-Century-Crofts, 1940. 18 citations.

ALLPORT, G. W. *Personality: a psychological interpretation.* New York: Holt, 1937. 18 citations.

POSTMAN, L., BRUNER, J. S., & MCGINNIES, E. Personal values as selective factors in perception. *J. abnorm. soc. Psychol.,* 1948, **43,** 142–154. 17 citations.

MILLER, N. E., & DOLLARD, J. *Social learning and imitation.* New Haven: Yale Univer. Press, 1941. 16 citations.

SPENCE, K. W. The differential response in animals to stimuli varying within a single dimension. *Psychol. Rev.,* 1937, **44,** 430–444. 14 citations.

BRUNER, J. S., & GOODMAN, CECILE C. Value and need as organizing factors in perception. *J. abnorm. soc. Psychol.,* 1947, **42,** 33–44. 13 citations.

FLESCH, R. A new readability yardstick. *J. appl. Psychol.,* 1948, **32,** 221–233. 12 citations.

We may perhaps conclude, then, that if we consider frequency of citation alongside a psychologist's bibliography, we have an instrument for facilitating the assessment of the influence of a psychologist and of identifying his most influential work.

*Received December 27, 1954.*

# APPLICANTS FOR FELLOW STATUS: 1957

Listed below are the names of 126 Associate members of the APA who are applying for Fellow status, together with the names of the divisions through which they are applying. The list is printed here in accordance with Council action of September 1952 revising procedures for Fellow applicants.

No action has been taken on these applications by the divisions, nor by the APA Membership Committee.

It is from this list that the divisions will make their nominations. By August 1, 1956 each division will submit its preliminary nominations. At the annual APA convention in September 1956 each division will submit its final list of nominees after the divisional business meetings. These final nominations, together with recommendations from the APA Membership Committee, will be considered by the Board of Directors, and a list of Associates recommended for transfer to Fellow status will then go from the Board to the APA Council of Representatives for vote. Fellow status for those elected will become effective January 1, 1957.

The deadline for filing applications with the APA office was January 1, 1956 for applicants who wished consideration at the September 1956 annual meeting. The deadline for those wishing consideration at the September 1957 meeting will be January 1, 1957. By that date applicants will need to file with the APA Central Office (ATTN: Membership Committee) one copy of the Uniform Fellow Blank for each division through which they are applying. The necessary blanks and instructions should be obtained from the appropriate division secretary (listed on the inside front cover of the November 1955 *American Psychologist*).

Albee, George W., SPSSI, Clinical
Angelino, Henry Richard, Developmental, Educational
Auld, Frank, Jr., Personality and Social, Clinical
Austrin, Harvey R., Personality and Social, Clinical
Back, Kurt W., SPSSI
Bahrick, Harry Phillip, Experimental
Bair, John T., Counseling
Baker, Howard D., Experimental
Barr, Estelle DeYoung, Personality and Social
Ben, Dorothy Yvonne, Clinical
Bernstein, Lewis, Clinical
Bier, William Christian, S. J., Personality and Social
Bills, Robert E., Teaching, Counseling
Blau, Theodore Hertzl, Developmental, Clinical
Blum, Lawrence Philip, Counseling
Bovard, Everett Warner, Jr., Personality and Social
Boyle, Ruth C., School
Brown, John Marshall, Teaching
Bucklew, John, General
Burnett, Collins Walter, Counseling
Chalfen, Leo, Clinical
Clendenen, Dorothy Marguerite, Counseling
Cohen, Jozef Bertram, Experimental
Cowen, Emory L., Personality and Social
Dakan, Everett A., Clinical
Dingman, Paul R., Clinical
Dixon, James Cannon, Clinical
Donofrio, Anthony F., Clinical
Dörken, Herbert Oliver, Clinical
Downie, Norville Morgan, Evaluation and Measurement
Drucker, Arthur J., Industrial and Business
Dubin, Samuel Sanford, Industrial and Business
Eells, Kenneth Walter, Evaluation and Measurement
Eriksen, Charles W., Experimental
Eson, Morris E., Developmental
Evans, Richard I., SPSSI
Farberow, Norman L., Clinical
Fleishman, Edwin Alan, Experimental, Evaluation and Measurement, Industrial and Business
Frank, Jerome David, SPSSI
Friedman, Samuel H., Clinical
Gasson, John A., Personality and Social
Gellman, William, Counseling
Gilchrist, Jack Cecil, Experimental
Gilson, John Francis, Educational
Glaser, Robert, Evaluation and Measurement
Gleser, Goldine Cohnberg, Evaluation and Measurement
Goldman, George David, Clinical
Grant, Claude W., Counseling
Grauer, David, Clinical
Grey, Alan Lewis, Clinical
Grice, G. Robert, Experimental
Grier, Mary Elizabeth, Clinical
Gruen, Walter, Personality and Social
Hanchett, Gertrude Aby, SPSSI
Harvey, Herman, Teaching
Hayakawa, Samuel Ichiye, SPSSI
Heron, Alastair, Evaluation and Measurement, Personality and Social, Industrial and Business
Ho, Ching-Ju, Counseling
Horowitz, Milton William, SPSSI
Horwitz, Murray, SPSSI
Houston, Clifford Granville, Counseling
Iscoe, Ira, Developmental, Clinical
Johnson, Elizabeth Zalesky, Clinical
Johnson, Walter Frank, Jr., Counseling
Kahn, Theodore C., Teaching
Katz, Melvyn Myron, Clinical
Kilpatrick, Franklin Peirce, SPSSI

Klatskin, Ethelyn Henry, Clinical
Krasner, Leonard, Clinical
Levine, Abraham Seth, Evaluation and Measurement
Little, Kenneth Bruce, Clinical
Livson, Norman, Personality and Social
Luft, Joseph, Clinical
Lyons, Anita Frances, School
MacCorquodale, Kenneth, Experimental
Mausner, Bernard, Personality and Social
McLaughlin, Edward James, Clinical
Meltzoff, Julian, Clinical
Meyer, Herbert H., Industrial and Business
Meyerson, Lee, Clinical
Millard, Kenneth Albert, Counseling
Milne, Bentley B., Counseling
Morrow, William Robert, Personality and Social
Mussen, Paul Henry, Developmental, Personality and Social
Niven, Jarold R., Industrial and Business
Noble, Clyde Everett, Experimental
Nuckols, Robert C., Industrial and Business
Orr, David Hamilton, Clinical
Pastore, Nicholas J., SPSSI
Perkins, Hugh Victor, Developmental
Pollack, Irwin, Experimental
Pomeroy, Donald Silas, Clinical, Maturity and Old Age
Raab, David H., Experimental
Ratoosh, Philburn, Experimental
Reid, Jackson Brock, Experimental, Educational
Remple, Henry D., Clinical
Riecken, Henry William, Jr., Personality and Social
Robinson, Elsa E., SPSSI
Rosen, Ephraim, Personality and Social
Royce, Joseph Russell, Experimental, Evaluation and Measurement
Sakoda, James Minoru, SPSSI
Salzman, Samuel, Counseling
Schaul, Martin W., Industrial and Business
Schneck, Jerome M., Personality and Social
Seeman, Julius, Counseling
Sherman, Harry, Industrial and Business, Counseling
Silverman, Hirsch Lazaar, School
Siple, Howard L., Clinical
Small, Leonard, Counseling
Smith, S. June, School
Soskin, William S., Clinical
Stellar, Eliot, Experimental
Steinzor, Bernard, Clinical
Stouffer, George A. W., Jr., School
Stubbins, Joseph, Counseling
Szyrynski, Victor, Clinical
Thibaut, John Walter, SPSSI
Vaccaro, Joseph John, Clinical
Vorhaus, Pauline G., Clinical
Weiss, Herman Robert, Clinical, Public Service
Wiebe, Gerhart David, Industrial and Business
Williams, Helen Elizabeth, Clinical
Witherspoon, Ralph Leo, Developmental
Wolfe, Ranald Milton, Clinical
Wrigley, Charles Frederick, Evaluation and Measurement
Yedinack, Jeanette Greenough, School
Zuckerman, John Vitto, Industrial and Business

# Comment

### Psychologists, Heal Thyselves!

The June 1955 edition of "Across the Secretary's Desk" (*Amer. Psychologist*, 1955, **10**, 255–257) contains some reports of actions taken at the spring meeting of the Board of Directors relating to official policy concerning disciplinary actions against members accused of breaches of professional ethics which are very disquieting. While recognizing that the Secretary's abbreviated reporting of the meeting probably does not reflect accurately the true flavor of the deliberations, the general tenor of the actions taken sounds remarkably similar to that of an academic disciplinary committee judging the fate of students detected in infractions of the honor system. The whole atmosphere seems moralistic, judgmental, and punitive rather than constructive, rehabilitative, and healing. Indeed, in their handling of administrative and interpersonal relations with their colleagues, many psychologists completely seem to ignore any responsibility for applying accepted clinical principles and methods.

In this and other connections, it has been noted that psychologists seem overconscientious and even compulsive in their efforts to be simon-pure and scientific almost to the point of fetish. Colleagues suspected of indiscretions are ostracized and avoided. We seem to be trying to identify ourselves with all that is "safe" and acceptable in contemporary codes and credos. Perhaps all this is a function of our youth and feelings of professional insecurity. Young sciences have to be respectable in order to be accepted by older more established disciplines. In this connection, it is significant that organized medicine does not deal so harshly and stringently with offenders. Before the final adoption of any standard procedures for handling ethical infractions, the APA would do well to study the time-tested methods of the American Medical Association which work remarkably well even though not widely understood and sometimes criticized as being too lenient.

Ethical infractions are usually the result of inexperience, poor judgment, or behavior pathology and should be treated as such. Before taking final disciplinary action, the profession has a responsibility to make determined efforts at rehabilitation of offenders. This is best accomplished on local levels through state or county organizations, and with disciplinary action being taken on national levels only after a complete breakdown of therapeutic efforts through noncooperation or refractoriness of the subject. Some of the actions and policies reported seem to disregard the possibility that persons guilty of ethical infractions might ever again become mature, law-abiding citizens. Such actions as (*a*) wide publication of the fact that a named member has been expelled from the APA, (*b*) to inform complainants, local organizations, and original endorsers of the person's membership application, or (*c*) informing present employers and institutions which gave the person professional training, are extremely drastic steps which could effectively terminate any further professional usefulness whether or not such person ever becomes more mature and responsible. Fortunately, the Board of Directors seems to have recognized the serious issues involved in their further action of voting further study of administrative policies and procedures.

All of this is highlighted by reports of the amounts of time invested by the Board of Directors at the recent San Francisco convention in discussing ethical infraction cases. Our profession has more important problems than the handling of ethical infractions, and the valuable time of the Board of Directors should not be tied up in judicial functions. Even though the APA membership has been highly screened, indoctrinated, and therapized, it may be expected to produce predictable incidences of errors of judgment and behavior deviations. The brusque expulsion of an offender is not a constructive action, and may engender paranoid attitudes while leaving him free in society to operate in even more unrestrained fashion. How much better to retain the offender in our midst while laboring to convert him to more responsible ways. We suggest that such therapeutic activity be included in the duties of committees on professional ethics, and that only persistent offenders be referred for action of the Board of Directors. We have little doubt but that patient friendly guidance by colleagues would prevent recurrence of 95 per cent of ethical infringements.

FREDERICK C. THORNE
*Brandon, Vermont*

### On the Issue "What Is a Science?"

I would like to express my very strong feeling that the entire argument of whether or not any particular profession or occupational area is or is not a science is invalid and needless. I submit that there is no such thing as *a* science; only Science, with a capital "S."

Science is a method and a philosophy and is not related to whichever area it is applied. A true Scientist may apply himself to any problem whatsoever without lessening his position as a Scientist. There are no such things as sciences; there is only Science and there are only Scientists. Properly defined, the word science, as it refers to an occupation or area of work, merely means that it is a field in which Scientists apply themselves.

It is true that as we approach new frontiers and new problems we are forced to be subjective and use units of measurement that leave a lot to be desired. But a true Scientist uses his judgments, intuitions, and inadequate units of measurement for what he can get out of them, so that he can set up new hypotheses that may be tested by more adequate methods. During this process he never forgets that his methods and conclusions are tentative and that he must strive for something better. However, by the same token, he does not reject that which has value and/or practical application just because it was gathered by "unscientific" means.

Our present-day thinking seems to say, for example, that a scientist—a person dedicated to the Scientific Method—cannot attack a problem area such as Personality because he cannot measure his variables by the deflection of a needle or the tipping of a balance. People who work on such problems are not Scientists because the field of Personality is not "scientific." If such were the case, scientific knowledge would be generations behind what it is now.

Again I say: there is no such thing as *a* science; there is only SCIENCE and there are only SCIENTISTS.

FREDERICK L. MCGUIRE
*Naval Medical Field Research Laboratory*
*Camp Lejeune, North Carolina*

## A Missing Variable

The participants in the discussion of the isolated worlds of the clinician and the experimentalist in the April, 1955 *American Psychologist* were penetrating in their observations, but they seemed to have left out one very important variable. One may "use" feelings, intuition, and fantasies in the same way that one may "use" experimental findings and methodologies, statistics, and scientific principles. Both statistics and intuition, and any other human sensitivity or invention, may be "used" to maintain one's omnipotence. A most crucial variable in the discussion is maturity. To be open to all that is reality will permit the clinician and the experimentalist to communicate and contribute mutually and cooperatively to our increased understanding of human behavior and perhaps make "the experimental clinician" not such a paradox.

HOWARD SHEAR
*The University of Texas*

## Rebinding Directories

There are perhaps other members of the association who share my reluctance to discard the handsome sturdy cloth-bound covers of the 1951 Directory. Although many of our colleagues may have disposed of their old directories, I would urge that those who have not done so be reminded that the hard covers can easily be substituted for the paper covers of the paperbound directory. Those who are adept at repairing and rebinding books might suggest a preferable way of doing it. The following method, however, yielded satisfactory results for me:

The front cover and flyleaf of the old directory were opened and the flyleaf separated at the joint with firm pressure from the thumbnail. The back cover and flyleaf were treated similarly, followed by separation of the super (gauze backing) from the spine using pressure from a blunt table knife. After removing the paper covers and applying a liberal coat of glue to the spine, the new directory was pressed firmly against the super of the hard covers. The spine of the newly-covered book was pressed firmly against the surface of a desk and other books were wedged on either side to hold it in an upright position overnight. Later, the flyleaf joints were touched up with a few more drops of glue and the date on the cover was made current with the help of some gold paint from a model airplane kit. The resulting volume makes a very presentable addition to my library.

ROBERT L. CARLTON
*Children's Mental Health Center*
*Columbus, Ohio*

## Comments on Rogers' "Persons or Science"

Rogers' article (*Amer. Psychologist*, July, 1955) is in many ways an admirable analysis, but I should like to submit that an important factor in his dilemma arises from his particular conception of psychotherapy and the role of the therapist. If indeed the therapist's participation in the interpersonal venture of psychotherapy is reduced to a totally subjective experience, the antithesis of objectivity vs. subjectivity becomes an acute one. As Rogers himself concludes, it does not have to be this way. Let me be more specific.

Speaking as a clinician, Rogers regards the therapeutic process, as experienced by the therapist, as a thoroughly subjective phenomenon. He uses such emotionally charged terms as "trance-like," "fullness of experiencing," " 'out-of-this-world' quality," etc. to characterize the situation. The same tenor is expressed by the client-centered therapist in "The Case of Mrs. Oak": "To me the thought of trying to guide or direct such an intricate human process is literally abhorrent, and I never felt any such impulse in regard to Mrs. Oak. To try to understand her, to go with her on the paths she was exploring, to let her feel the acceptance I experienced toward her, this seemed to be a fully satisfying task to me" (2, p. 264).

Every therapist with empathic capacities (and there should be none without them) has undoubtedly felt something of this ineffable experience which Rogers

has attempted to put into words, and will regard it as a *sine qua non* of the psychotherapeutic process. The question, however, arises: Is this the *essence* of psychotherapy as we know it today, or is it merely a precondition, however indispensable? Rogers, of course, recognizes that a new kind of emotional learning takes place within this matrix, but he asserts that "The most that one person can do to further it in another is to create certain conditions which make this kind of learning possible." It is undeniably true that learning in psychotherapy, as probably in most situations, is in the final analysis self-learning. The verbal exchange in psychotherapy, from beginning to end, is placed in the service of effecting such learning. We can agree with Rogers that the patient's verbal description of this process, once he has experienced it, is quite immaterial. The important thing is that he has felt it.

For the Rogerian therapist the creation of a benevolent emotional climate is quite enough. Now contrast this conception with one that does not deny the importance of this precondition, but which in addition assigns to the therapist an active, reconstructive, and re-educative task. Sullivan's description will serve as a good example. Commenting on the factors which bring about favorable change in a person, he sees the patient as having "been *restrained from using the totality of his abilities*. The problem of the psychiatrist in treatment is to discover what the *handicaps* to the use of his abilities are. . . . I try to find out why he *can't* do the simplest thing, and in such investigation may come to solve the problem (3, p. 237). . . . [The task is to] work toward uncovering those factors which are concerned in the person's recurrent mistakes, and which lead to his taking ineffective and inappropriate action. There is no necessity to do more" (3, p. 239).

The role of the therapist as more than a subjectively involved partner is made even more explicit in the following quotation: "From beginning to end, to the best of his ability, the psychiatrist tries to avoid being involved as a person—even as a dear and wonderful person—and keeps to the business of being an *expert;* that is, he remains one who, theoretically and in fact, deals with his patients only because he (the psychiatrist) has had the advantage of certain unique training and experience which make him able to help them. . . . He may feel that interviewing is hard work, as I recommend everyone should. It is, beyond perchance, very hard work" (3, p. 36).

What Sullivan calls respect for the patient, what Braatøy (1) (following Ferenczi) calls love, what Rogers calls acceptance—these factors must be present before therapy can begin. But does it end there?

Rogers' dichotomy between the therapist as a participant and as an observer appears to be somewhat of an artifact. It is difficult to see how a therapist who is completely subjectively immersed in the experience can be of constructive help to the patient. Freud, too, stresses the technical factors in the analyst's work. He likens the therapist to a surgeon who calmly employs his skill for the sole purpose of effecting a successful termination of the operation.

This emphasis on skill, expertness, work, far from making psychotherapy "directive" or reducing it to a mere technology, points to the rational nature of the therapeutic task, and the possibility of scientific explanation. It brings to the fore the importance of *technique,* with the implication of manipulation in Rogers' nonvaluative sense. Without skill, without technique, psychotherapy is unthinkable. It is the technique, and indirectly the scientific basis upon which it rests, which differentiates modern psychotherapy from spiritual guidance, the confessional, and similar procedures.

Technique implies the deliberate employment of skill, and introduces from a different vantage point objectivity and science into the relationship. As soon as the therapist observes what is transpiring in the therapeutic situation—and I am convinced that he should never stop doing so—he brings the process of therapy closer to objective description and scientific understanding. Implicit in the activity of the therapist is a constant shifting between the role of participant and the role of observer. Freud's singular ability to objectify the subjective elements of his therapeutic experience is probably the best case in point. Ideally, each psychotherapist should function in both roles.

We may deplore the slowness and crudity of our research efforts in this area, but there is no reason to conclude that the richness of the therapeutic experience is destroyed by our increasing success in bringing the scientific method to bear upon it. As our powers of observation are sharpened, rather the opposite should be the case.

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2. ROGERS, C. R., & DYMOND, ROSALIND F. (Eds.) *Psychotherapy and personality change.* Chicago: Univer. of Chicago Press, 1954.
3. SULLIVAN, H. S. *The psychiatric interview.* New York: Norton, 1954.

HANS H. STRUPP
*The George Washington University*

* * *

Rogers' article was painful in its implication for those who are now struggling for scientific method to clarify our present state of development. It could be more harmful to the graduate student who is looking for leadership in this field. How can such an integration as

Rogers' which reifies science, glorifies mute feelings of ignorance by calling them personal subjective values, and abounds in infallible premises be looked upon as typical of the clinician's or psychologist's viewpoint? I felt that it was unkind of him to expect me and perhaps others to react "organismically in terms of relationships which are not present in my awareness." At least I was not aware that I suffered the same dilemma as Rogers.

I do consider that dichotomies are the initial stages of refinement of variables. I believe that literature, practice, and research are far ahead of this phase of development in therapy. So it becomes very disturbing to realize others may accept Rogers' conflict as the current state of psychological thought. In fact, I can only interpret his method of dissolving this personal discomfort as a washing of his hands. I feel that especially in the interpersonal field much more than lip service to science is required of those in whom leadership is invested. I am certain that more is existent today than the hopeless conclusion provided by Rogers.

GEORGE F. CASTORE
*State Hospital South*
*Blackfoot, Idaho*

* * *

Comment on Rogers's article is irresistible, yet difficult and saddening. Difficult because we have respected Rogers as a creative leader. Yet what graduate student could get by with such talk of "the essence of therapy," "the subjective and the objective person," "the scientific versus the experiential viewpoint," etc.? Saddening because I know better than this, and yet because of my lethargy Rogers stands as a spokesman for psychotherapy.

It is my fault and the fault of all of us who know better that Carl Rogers speaks for psychotherapy in his painfully dichotomous language. For a long while we have been assuring ourselves that we have advanced beyond the mind-body, the subjective-objective, the emotional-cognitive, the consciousness-organismic, and other dichotomies which support the untenable structure within which Rogers dwells. For a long while we have been telling ourselves that we know how to abstract relationships from various reference points and to devise hypothetical constructs which have research value. If we did know, how would such articles as this occupy the lead spot in our professional journals?

The truth is, I think, that we can do better than this. After the shock is over, I am thankful to Dr. Rogers for hastening the shattering of complacency and lethargy which has impeded us. Our research team here at Blackfoot has learned to avoid the semantic dichotomies. For example, there are no such things as subjective data; as long as they're subjective, they aren't data. We have learned to write better hypotheses than Rogers has written. Ours are stated in operational terms with clear reference to behavioral events. His are not. For example, what are the references for "acceptance of the client"? What are the references for "acceptance of self"? These presumably could be determined. but Rogers takes them for granted. Reliability of raters on such matters, or a satisfactory Q sort, are not adequate to make up for the lack of an operational basis in behavioral terms. Finally, we have learned to score the responses of participants in psychotherapy in such a manner as to treat the manifestations of personality functioning in the same way that other data are treated in scientific processing.

The greatest disservice that Dr. Rogers does for psychotherapy seems to be his insistence on something mystical in the therapeutic process. There is nothing mysterious about the sources of this mysticism. His "inherent growth principle" or "drive toward self-realization" has always been mystical simply because he does not relate these mysteries to clearly denotable behavioral processes. There is another trace of mysticism in his seeming naiveté about learning. What happens in therapy, he says, is a type of learning that cannot be taught. Rogers may still believe that the nondirective or Rogerian therapist doesn't teach his client anything, but this is no longer so shocking to me. It underlines the "must" which our research team is tackling in its slow and lethargic way—the need for placing our studies of psychotherapy on a clearly interpersonal and interactive basis, for getting away from mysticism and hidden undefinable "principles," for assessing psychotherapy in terms of denotable behavioral events. These achievements are all within our grasp, but it is plain that some acceleration is indicated.

RICHARD A. LAKE
*State Hospital South*
*Blackfoot, Idaho*

* * *

Rogers' "Persons or Science? A Philosophical Question" is a most provocative and stimulating article. The statement of the issues between the scientist and the experientialist is incisively and clearly drawn. However, the integration of the contrary views expressed although very satisfying to Dr. Rogers leaves much to be desired.

Rogers states that science exists only in people. This is a partial truth—science not only exists in people but its findings become institutionalized. This does something to people and their society whether or not they perceive as the scientist does. Homo sapiens 1955 must respond in some way to the airplane, atomic energy, antibiotics, and modern plumbing regardless of his scientific understanding of their development. To the Ubangi confronted by guided missiles the subjective

struggle of the scientist is very unimportant but the product is of vital concern.

To Rogers, "It is clear that scientific findings can be communicated only to those who have agreed to the same ground rules of investigation." At the same time his product and findings may irrevocably change the social structure of a given group with or without such agreement. After all the percentage of scientists in any given population is extremely small.

However, these points are small compared to the major thesis of the article that the more open one is to experience the more likely the person is socially constructive. This hypothesis requires concrete demonstration. Biologically speaking there are cogent arguments for the antithesis of this point of view. There is reason to believe that if the organism was totally open to experience complete inactivity and destruction would result. If we could hear all, see all, feel all, sense all, action and reaction would be an impossible matter. The homeostatic balance is as much based on eliminating certain experience and selecting certain experience as upon opening and making vivid a wide range of experience. If by openness, Rogers means, selective awareness then nothing new has been added. But if he means, as he says, "all the sensings of my intricate organism to be available to my awareness" then this is not only impossible but probably nihilistic. Further, we have little basis to assume that such "sensing" would be automatically good for society. Freud was not nearly as optimistic about unchecked id impulses as Rogers seems to be.

In some aspects it would appear that for Rogers religious dogma has been supplanted by an existentialistic dogma. This is mindful of Russell's admonition, "The method of 'postulating' what we want has many advantages; they are the same as the advantages of theft over honest toil."

REED M. MERRILL
*University of Utah*

* * *

Rogers' thoughts and worries concerning the scene of American psychology parallel closely my own. His three basic points are:

1. That the general climate of American psychology discourages original and creative thinking, and does little to foster it.
2. His second concern is with the risk of American psychology becoming parochial and narrowing the field of psychology.
3. A real fear of thinking about what psychologists are doing.

It may possibly have escaped Rogers that all three points have a common denominator—namely that of *constriction*. And, if one keeps in mind his remarks about the overgrowth of criticism and rigorous technique in distinction to creation and free thought, one is tempted to think of the prevailing temper as one of *obsessive-compulsive attitudes*. Since these tendencies are ego syntonic, one might think of it as something like an obsessive-compulsive character neurosis of the profession.

I have long been impressed with this phenomenon; back in 1941 I recall a seminar in which one of the outstanding European teachers (who has had a considerable effect on the American psychological scene) addressed a group of graduate students. Several of the brighter boys tore his statements to the shreds and tatters Rogers mentions. I must say, I was very proud of my colleagues, and only some time later was I duly impressed that, nevertheless, this elderly man had given some very useful hypotheses and stimuli to research and had advanced psychology by his own work. And today, almost one and a half decades later, I am afraid I cannot find a single original contribution to hold to the credit of my erstwhile colleagues (though they are all well-known, institutionally accepted, and well-placed psychologists).

In our American psychological culture, there is a great deal of value invested in caution, "cleanness," conformity, reasonable eclecticism, and emphasis on method. An anecdote F. L. H. Wells, I believe, once told me, expresses the problem better than anything else: He likened German (and for that matter, European psychology) to a man who knows nothing about swimming but jumps into the deep water and flails around wildly in his attempts to stay afloat. The American psychologist can be compared to a man who takes careful swimming lessons on land—and never goes into the water at all.

Now, obviously, the simile (which could also be applied to clinicians vs. academic psychologists) is a device to sharpen contrasts and by that token distorts and does some injustice, but it is useful. Naturally, neither of the above extremes of attitude is good. In another context (Bellak, L. *The T.A.T. and C.A.T. in clinical use*. New York: Grune & Stratton, 1954) I have spoken of the *oscillating* function of the healthy ego: it must be able, in order to function flexibly, to test reality at one moment, and exclude its critical function in the next instant—and thus oscillate back and forth. It would be a mistake to identify the scientific attitude with the obsessive-compulsive one. Surely, careful testing, detachment, caution are necessary attributes of a scientist. But, if they are not coupled with the "conception, birth, and nature of ideas" which Rogers speaks of, all we have is a technician. By the same token, of course, if all we have is free-floating ideas, and no need to check them carefully against reality in methodologically acceptable ways, all we have is dreamers, mystics, and eccentrics.

There is so much value placed on the obsessive attitude in American psychology that the methodologists, the "purists" have a great deal of disdain for those who permit themselves some ideas or are, of necessity, empiricists (like the clinicians). The attitude is not much different from the one of being the true children of the covenant—or maybe their attitude could be more usefully likened to puritans who feel that their straight and narrow and clean path is the only one leading to salvation. Even the emotional charge of an idea (without which creativity is often not possible) constitutes contamination. Hence, eclecticism. What may well happen to such an eclectic science is brought out in a favorite poem of mine, the author of which I have not been able to ascertain—and which is therefore, reproduced here with apologies for the lack of acknowledgment:

> Behold the mighty dinosaur
> Famous in prehistoric lore.
> Not only for his power and strength,
> But for his intellectual length.
> You will observe by these remains,
> The creature had two sets of brains.—
> One in his head (the usual place,)
> The other in his spinal base.
> Thus he could reason a priori
> As well as a posteriori.
> No problem bothered him a bit
> He made both head and tail of it.
> So wise was he, so wise and solemn,
> Each thought filled just a spinal column.
> If one brain found the pressure strong
> It passed a few ideas along.
> If something slipped his forward mind
> 'Twas rescued by the one behind.
> And if in error he was caught
> He had a saving afterthought.
> As he thought twice before he spoke
> He had no judgement to revoke.
> Thus he could think without congestion
> Upon both sides of every question.
> Oh, gaze upon this model beast,
> Defunct ten million years, at least.

So much for the dangers of excessive rigidity and eclecticism; as to *the therapy* for the condition diagnosed by Rogers and myself?

I doubt very much that Rogers' suggestion of a course on "Conception, and Nature of Ideas" will be sufficient. I am sure Rogers would not believe that a lecture can cure a pathological condition. We will have to apply mental hygiene, and bring up psychologists differently, in a way less likely to produce obsessive-compulsive characteristics (though the genesis of the problem may transcend psychological education).

It will probably take a consistent teaching attitude that stresses the positive values of hypotheses as much as their shortcomings; it will take frequent seminars dedicated to the unhampered discussion of ideas; it will take a fundamental change in editors' attitudes toward submitted manuscripts, and a basic change in book reviews, and a more permissive attitude toward ideas generally, to bring about a change.

About a year ago, I had a fascinating discussion with a well-known psychologist about some ideas of his. He volunteered that he would never dare publish them as long as he wants an academic job, since they were not of an experimental nature. A colleague of mine refused to write a paper concerning some general deductions and applications from a careful piece of experimental work to related fields, saying that he had a "real horror" for such unsupported generalizations. An editor wrote that he was immediately interested in a manuscript submitted, but finally decided that he had to reject it because "it was too much of a think-piece" (despite the fact that it not only outlined a hypothesis but also submitted an experimental outline for testing it). Book reviews are far too much dedicated to showing the other guy wrong—often in a stereotyped way complaining about lack of experimental support! I saw that happen recently in a really foolish way with a book written for medical practitioners, clearly marked in the subtitle and the foreword as a work of applied science which had to be kept simple for the use in general practice. Nevertheless for paragraph after paragraph the experimentally minded reviewer (who could justifiably have dismissed the entire book in a few sentences as not of value to academic psychologists) kept pouring out venom about its methodological shortcomings.

Let us hope that the APA will set up a travelling committee concerned with standards of "creative atmospheres" as much as there are committees investigating the technical status of departments of psychology. *Both* are necessary!

LEOPOLD BELLAK
*New York, New York*

# *Across the Secretary's Desk*

Among the livelier things crossing the Secretary's desk in recent weeks was a piece by John B. Carroll dealing with Rudolf Flesch's book *Why Johnny Can't Read*. At first blush, it appeared that the publication of the piece would probably represent the beating of a dead horse. But no. Flesch is still being dealt with in newspapers and magazines. And the mention of either Flesch or Johnny is enough to precipitate a rise in the decibel level of almost any conversation, even conversations among psychologists. So, because the horse is still alive, because it would probably be dead by the time Dr. Carroll's piece could be published through usual channels, and for some other reasons, the piece is given this space.—F. H. S.

### The Case of Dr. Flesch

On page 123 of his recent best-selling book *Why Johnny Can't Read* (11), Rudolf Flesch makes the following statement:

> There are only two kinds of experts worth listening to when it comes to reading: linguists and psychologists.

Flesch takes pains to give the impression that he is extremely suspicious of educators, at least educators who have done research on methods of teaching reading, for these are presented as being engaged in a conspiracy to "conceal . . . the true facts" (p. 61), even (he implies) to conceal or ignore the results of their own research studies (p. 68). Educators who have lent their names to series of readers used in elementary schools, or who teach teachers how to teach reading, are especially not to be trusted, Flesch would have us believe.

In view of all this, it is with some trepidation that I undertake to comment on the now celebrated controversy about whether Johnny can read, for there is a possibility that Flesch would class me, along with the current President-elect of the APA (whom he takes to task on page 43), among those "educators and teachers' college professors who happen to be members of the American Psychological Association" (p. 124). On the other hand, I do not think I have at any time been involved, ego or otherwise, in any system of teaching reading. I am not the author of a series of readers, nor do I teach the teaching of reading (except as it may occasionally come up for discussion in seminars on the psychology of language). If anything, I am on record (8, pp. 146–150) as having been mildly critical of some educators' views on reading instruction, and even slightly sympathetic to the "linguistic" approach which Flesch claims to recommend. In any event, I do not feel that any psychologist need feel apologetic about being affiliated with a "teachers' college," where there is as much opportunity as anywhere else for "scholars whose main work is the study of the human mind"—as Flesch characterizes (p. 124) the kind of psychologist who is "worth listening to." I would, in fact, urge psychologists to line up with educators like Gray, Gates, Witty, and others in their defense against the attacks made by Flesch.

For the sake of both the linguists and the psychologists whom Flesch would have people listen to, it is extremely important to have the record set straight. A number of writers have already stated (4, 5, 14) that Flesch has distorted and misrepresented the research evidence concerning the teaching of reading, particularly the research on the role which "phonics" should play in this teaching, and that his accusations have needlessly distracted and embarrassed American educators at a time when the schools have their full share of real rather than fancied problems. It is ironic that this particular attack on educators should have come from one who himself once earned a PhD from a teachers' college.

Members of the American Psychological Association, in confronting this whole matter, may wish to assess Flesch's performance in terms of the following excerpt from the *Ethical Standards of Psychologists*, adopted in 1953.

> The public requires dependable sources of psychological information, and it is in the interest of the profession that the public be well supplied. Psychologists who interpret the science of psychology or the services of psychologists have an obligation to report fairly and accurately. Exaggeration, sensationalism, superficiality, and premature reporting of new developments should be avoided; modesty, scientific caution, and due regard for the limits of our knowledge should characterize all statements.

Dr. Flesch became an Associate member of the American Psychological Association in 1945; he must have resigned, or allowed his membership to lapse, sometime after 1953, for his name appears last in the 1953 APA Directory.

In getting into this whole matter the array of hypotheses to be examined include the following:

1. That Flesch is right and that most teachers of reading are wrong.
2. That Flesch has, willfully or otherwise, misrepresented research results on the teaching of phonics.
3. That Flesch can't read.
4. That Flesch does not understand elementary statistics.

In order to establish a basis for the analysis which I shall make, it will be useful to sketch briefly the major conclusions which any fair-minded reader can draw from the published research on the question of "phonics vs. no phonics."

First, a little terminological discussion. What is phonics? *Phonics* is not to be confused with *phonetics,* which is the science of speech sounds and has nothing necessarily to do with reading or spelling. It is careless to say that "English is an unphonetic language," as some educators and laymen do, for English (that is, *spoken* English) is just as "phonetic" as any other language, if we mean by that statement that its sounds can be studied scientifically. When educators speak of the teaching of *phonics,* they mean the teaching of relationships or correspondences (regular or irregular) between the sounds of the spoken language and the symbols (or sequences of symbols) which may be used to represent these sounds in the customary orthography or system of spelling. In English orthography, the sound-symbol relationships happen (for historical reasons) to be somewhat more irregular than in those of most other languages, and even the regularities are somewhat more complicated and "unreasonable" than they need to be, as pointed out by the would-be spelling reformers. "Phonics" can be introduced into the teaching of reading at various stages, either at the very beginning, or later. There are various ways of teaching phonics: it can be done in a somewhat mechanical way, teaching the letters of the alphabet and their sounds, then having pupils "sound out" words; it can be done inductively, by presenting children with sets of similarly pronounced words with common orthographic features; or again, it can be done in a rather "incidental" manner (still remotely inductive, however), simply by frequently pointing out the relation between a given sound and a given letter or combination of letters, the while using other methods of teaching reading. The mechanical way of teaching phonics is the "old-style," "conventional" phonics method originated many years ago; it tends to lead to constraints on the kinds of words and sentences which can be introduced in the early stages of reading. (The method of teaching reading advocated by Flesch may be characterized as most nearly resembling old-style, conventional phonics.) The "incidental phonics" method, on the other hand, typically allows flexibility in the introduction of a vocabulary better related to the children's interests and needs.

The opposite of "phonics" is often considered to be the "word," "sight," or "look-and-say" method, which attempts to develop immediate associations between whole words and their spoken counterparts, with no explicit attention to the separate letters or sounds composing these words. A number of other "methods" are described by Anderson and Dearborn (2); actually, all these methods may better be regarded as procedures which may be variously used and combined in a well-balanced reading program.

From the above, it becomes immediately obvious that it is misleading to draw the issue as between "phonics" and "no phonics," and many of Flesch's distortions of research evidence stem from his failure to inform his readers precisely what kinds of "phonics" instruction are involved in the research studies on which he reports.

The major conclusions to be drawn from the research evidence available are as follows:

1. In the first grade of the elementary school, the best plan to follow for the average child is to start the first half-year with various "readiness" activities and familiarization. As children develop interest in reading and appreciation of the use of the written word, they should be gradually taught to recognize a small "sight" vocabulary without necessarily learning any sound-symbol correspondences (9, 15, 20, 21, 22).

2. Possibly in the latter half of the first grade, and certainly in the second year, children will gain somewhat more if inductive or incidental phonics instruction is presented. Failure to introduce phonics in the second grade or later may adversely affect word-recognition skills and spelling (3, 18, 23).

3. Children who are taught *from the beginning* by a conventional, old-style phonic method do not do as well in various reading performances (par-

ticularly those involving comprehension) as those taught in the way described in (1) and (2) above; however, the differences are not dramatic, and may or may not be statistically significant (16, 21).

4. Children who are given *no* phonics at all in the first grade do almost as well as those given gradual, incidental phonics instruction, and usually better than those receiving "conventional" phonics instruction. They tend to develop phonic insights and generalizations by themselves (22).

5. The method of instruction makes very little difference for fast, apt learners; the superiority of "incidental phonics" over "no phonics" and "conventional phonics" becomes more marked for low aptitude pupils (15).

6. Many children move out of the elementary school grades without having acquired adequate word-recognition skills based on phonic knowledge; wide differences in "phonic ability" can be detected even at the college level. The extent to which the method of instruction is responsible for these differences is not known (23, 24).

7. Despite what may be taught in schools of education, or recommended in books about the teaching of reading, there are still fairly wide variations in the methods used by teachers, at least according to their statements (6, 19).

To judge from Flesch's Chapter V, which purports to be an exhaustive survey of the literature on "phonics" vs. "no phonics," there is only one issue: which method shall be used, the "phonic" method or the "word" method? He claims to find no evidence whatsoever favoring the word method, and the casual reader of the conclusions cited above might tend to agree with him. We have already commented on the fallacy involved in this reasoning. The reader might persist, however, and ask whether it might not be true that Flesch made an honest mistake in not recognizing that methods are inevitably complex and that there are different varieties of phonic methods and different stages at which the introduction of phonic teaching might or might not be appropriate. Bearing on this point is the bountiful evidence that Flesch misrepresented a number of research studies, going directly counter to their authors' own statements. Such is true for studies by Currier (9), Gates (13), Sexton and Herron (20), Mosher and Newhall (17), Garrison and Heard (12), Tate (21), Agnew (1), and Russell (18).

Start with the Currier and Duguid study (9), published in 1916. True, this is not a rigorous study by contemporary standards, but it was sufficiently well controlled to indicate a trend. According to Flesch, Miss Currier performed a small-scale experiment in a Tilton, New Hampshire public school. Still according to Flesch, "She had no statistics to offer but reported that the 'non-phonic' children read with more expression and interest, but the 'phonic' children were more careful and more accurate in reading the words there were on the page." If we let Miss Currier speak for herself, we learn: "The phonic classes were so concentrated on letter sounds that the attention was diverted from the *sense* of the paragraph to *word pronunciation*. . . . The reading was generally less smooth, slower, and the idea confused." Apparently, Flesch has a high tolerance for confused ideas.

Next let us note how Flesch handles the 1927 experiment of Gates (13), which he says was responsible for the trend away from phonics. According to Flesch (p. 54): "Dr. Gates sets up an experiment: one first grade is taught by his new 'incidental phonics'; another first grade—the control group—is exposed to conventional phonic drills. After a few months, the two groups are tested. Hurrah! The new method has won. And Dr. Gates is on his way to drive phonics out of American schools." On reading the Gates article, we by no means get the impression that Gates thought to himself, "Hurrah! the new method won!" On the contrary, none of the differences between the experimental and control groups were significant, a fact which Gates himself emphasized. There was only a weak trend favoring the "incidental phonics" group. (By present-day standards, the statistical methods used were crude, and not even the *N*'s were reported; nevertheless, there is little if any indication that the null hypothesis should be rejected in any of the comparisons.) Gates himself concluded: "In some of the earlier investigations, as suggested by such titles as 'Phonics or No Phonics' (cf. the title of Flesch's chapter!), it has appeared that there was no choice other than to accept or reject the complete phonetic system. The intelligent procedure is to determine what phonetic drills, devices, etc., are of value."

We now come to the research of Sexton and Herron (20) published in 1928, in reference to which Flesch says (p. 62): "In spite of a rather

confusing experimental set-up, they concluded that the results favored instruction in phonics." The experimental set-up would not be confusing to anyone who understands experimental design. In any case, here is what Sexton and Herron actually said: "The teaching of phonics functions very little or not at all with beginners in reading during the first five months. It begins to be of some value during the second five months but is of greater value in the second grade." They point out that ". . . teaching ability has immeasurably more influence on the teaching of reading than has the use or non-use of phonics."

Flesch next reports on the experiment of Mosher and Newhall (17), published in 1930: "Fifty children in New Haven, Connecticut were taught by the word method and seventy-three children by the phonic method. The two groups were given ten tests. Eight of the ten tests favored phonics." True, but if you read the Mosher and Newhall report you will find that none of the differences were statistically significant, and that the two differences which favored the "word method" were the largest of all.

According to Flesch, Garrison and Heard (12) "experimented with about one hundred school children in first and second grade; one half had phonics, the other half had none—or rather, they had the so-called 'intrinsic' phonics invented a few years earlier by Professor Gates of Teachers College, Columbia University. At the end, there was a series of tests. Total result: The phonics group scored 58.5, the other group 55.5. Three points in favor of phonics." Flesch ignores the fact that the PE of this difference is reported as 1.53, a figure which allows us to find that the probability of the chance occurrence of a difference as great as three points is about .19, or nearly one in five. (Garrison and Heard did not report the probability level, but surely Flesch could have done the simple computations and table-referrings required.) One of Garrison and Heard's conclusions reads: "In the teaching of reading it seems probable that much of the phonetic training now given should be deferred till the second or third grades. It appears that work in meaningful exercises which are planned to increase comprehension and to teach discrimination of words is more important than phonetics." I shall not bother to quote other conclusions which Flesch does not include.

The 1937 experiment of Tate (21), to read Flesch's discussion of it, sounds like iron-clad evidence on his side. As he reports it:

> "A group of 36 first-graders were taught by the look-and-say method, another group of 37 children were given exactly the same instruction plus 15 minutes each day of drill and practice in phonics. After two months they were given three tests. Two of the tests ('silent reading' and 'paragraph reading') were tests of guessing rather than reading and the word-method children scored slightly better. The third test, however, was a test of 'word recognition.' In this test the score of the phonic group put them 4.6 months ahead of their 'normal reading age,' which means, according to Mr. Tate, that they scored *270 per cent better* than the word-method group. . . . Mr. Tate comments that this result is 'overwhelming proof of a reliable finding' and adds: 'Phonetic instruction and drill, as judged by the results of the Gates Primary Reading Test, Type 1, is far superior to the look-and-say method in developing the ability to recognize words.'"

Let us now report what Tate actually said. To be sure, Flesch did quote Tate accurately, but the quotations were pieced together in a rather forced manner from various portions of the article, and the conclusion cited above was only *one* of Tate's conclusions. Tate queried the teachers of his phonics group, who pointed out that the phonic instruction tended to hold back progress and make some children use their lips too much in silent reading. Tate also laid stress on the results of the sentence-reading and paragraph-reading tests, which favored the non-phonic group. His final conclusions were as follows: "Regular periods for phonics instruction and drill are not desirable. Phonics should be used by the pupil as a tool and not as subject matter to be mastered for its own value." Flesch does not report the fact that this was how Tate really felt about phonics.

Next in chronological order would come the experiment of Dolch and Bloomster (10) on "phonic readiness," which Flesch takes up in his Chapter VI. He calls this the "one single experimental study in which the onset of phonic readiness at seven was discovered" (p. 72). This may be the case, and it is also possible that some educational authorities have misinterpreted this study, as Flesch claims. At any rate, what the experiment *does* show is that ability to make phonic generalizations *without training or guidance from the teacher* probably does not mature until a mental age of seven or greater. The experiment tells nothing, contrary to what Flesch seems to imply, about whether or

not children can be taught or guided to use phonics before a mental age of seven. The weight of evidence from other studies suggests that they can.

I have not yet been able to examine in the original Agnew's study (1), which Flesch presents as favoring the conventional phonics approach, but from Anderson and Dearborn's (2) discussion of it I judge that Flesch made too much of small, insignificant differences which appeared to favor conventional phonics, and that he played down too much the clear finding that the non-phonic group read faster on the average. I shall not make an issue of Flesch's treatment of this study, however.

We come now to the last experiment discussed by Flesch, a study by Russell (18) published in 1943. Because it suited his purposes, Flesch reported this particular experiment with reasonable accuracy, but the interpretation of the results hinges on semantics. The children who got "phonic work on sounds" did better on tests than children who got "little or no phonics," but this does not exclude the possibility (in view of the investigator's known views, a high probability) that the "phonic work on sounds" was more in the nature of the "incidental phonics" which had been found effective in other experiments, and not at all the old-fashioned phonic drill advocated by Flesch.

In setting out to make his survey of the literature on the "phonics vs. no phonics" question, Flesch claims he attempted to track down "every single reference." He continues (p. 61): "I carefully read each one of those papers and monographs. Naturally, it is possible that some item or items in the bibliography have escaped me; but I don't honestly think so. I covered the ground as diligently as I possibly could, looking for scientific evidence *in favor* of the word method. . . . There was none." At the end of his literature survey chapter, the like of which has probably never before appeared in a best seller, he tells us (p. 68): "The story as I told it here is complete; this is the sum total of all experiments ever made. I have left out nothing and I have misrepresented nothing—to the best of my ability as a researcher."

The evidence cited above bears on the question of Flesch's misrepresentation of research findings. There is also evidence on Flesch's ability to handle simple statistical data. What about his statement that he "left out nothing"? When I started to check the completeness of Flesch's coverage, I was in much the same position that Flesch seems to have been in, for I had never had occasion to make an exhaustive literature search on this particular problem. With very little difficulty I was immediately able to locate a number of important and pertinent studies. I found that Flesch had completely missed the study of Gates and Russell in 1938 (15), which shows a slight superiority of an "incidental phonics" method over *both* the word method (using little or no phonics) and a method emphasizing conventional phonics—a superiority which is more marked for pupils low in reading readiness. I found that Flesch had omitted the study by Tate, Herbert, and Zeman (22) which found that "incidental phonics" was slightly superior to a method in which all phonics instruction was deliberately excluded. The study by Burt and Lewis (7), published in England in 1946, and casting doubt on the usefulness of phonics instruction for "backward readers" (who *do* turn up in England, despite any impression which Flesch may have given to the contrary), was completely passed by in Flesch's "exhaustive" search, as was also the recent and very pertinent study by McDowell (16). McDowell's study, conducted in parochial schools in Pittsburgh and vicinity, compared a rather systematic "phonetic" method with the method which was regularly used in these schools, and found that the regular method generally yielded slightly superior achievement after three years. (It can be assumed that the regular method itself contained some phonics instruction.)

We shall excuse Flesch for failing to mention two pertinent studies published in 1954 (3, 23) on the chance that they appeared after he submitted his manuscript.

If one could search the length and breadth of the land, one could probably locate a sizable minority of teachers and educational administrators for whom "phonics" is almost an unmentionable word, and who in consequence may be doing less well with their charges than they might do. One might find also a considerable number of teachers and educational administrators who believe, contrary to the research results, that a strict phonic approach is the most efficacious method of teaching reading. If Flesch had wanted to make a constructive contribution to the teaching of reading, he might have used his talents of persuasion and rhetoric in the cause of

drawing the attention of these groups to the true importance of "phonics" as *one* of the procedures to be followed in a well balanced program of reading instruction. He could also have performed a much needed service by addressing himself to the parents who are over-anxious about Johnny's difficulties in reading, telling them how they might better cooperate with Johnny's teacher and school authorities, and demonstrating for them some *tested* techniques in helping Johnny learn to read. There is even a possibility that Flesch could have shown teachers how a "linguistic" approach might improve present-day techniques of teaching phonics.

If Flesch had chosen to do these things, instead of what he did, he could still have produced a best seller. The present analysis will give psychologists and their friends the bases for recommending a fate for the best seller under scrutiny here.

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JOHN B. CARROLL
*Harvard University*

# *Psychological Notes and News*

**Robert M. Yerkes,** professor emeritus at Yale University and president of APA in 1916, died on February 3, 1956 at the age of 79.

**Warner Brown,** professor emeritus at the University of California, died on February 3, 1956 at the age of 75.

**Grace E. Bird** of Providence, Rhode Island, died on December 1, 1955.

**Geraldine Brundage** of Falls Church, Virginia, died in December, 1955.

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**Joseph Nuttin** of the University of Louvain, Louvain, Belgium, will join the staff of the department of psychology at the University of Kansas as a visiting professor for the spring semester of 1956. He arrived on March 5 and will remain in Lawrence until May 26. He is replacing Martin Scheerer who is on sabbatical leave.

**John J. McMillan** and **Louis A. Wienckowski** have recently joined the Research Grants and Fellowships Branch of the National Institute of Mental Health in Bethesda, Maryland. Dr. McMillan was formerly a clinical psychologist at the VA Hospital in Richmond, Virginia, and Mr. Wienckowski was formerly an associate research psychologist at the Human Resources Research Office in Washington.

Convair, San Diego, announces that **Melvin Freitag** and **David Meister** have joined its Missile Reliability Group. They will study human engineering problems involved in the design and operation of guided missiles.

**Chester W. Harris** of the University of Wisconsin School of Education has been named editor of the third edition of the *Encyclopedia of Educational Research.* This edition, due to be published in 1960, will bring together the main findings of research scholars in education since the second edition, published in 1950.

**Mario Levi,** formerly research psychologist with the Air Force Personnel and Training Research Center, Survival Field Unit, Stead Air Force Base, Nevada, has resigned to accept the position of Chief, Personnel Research and Measurement Section, Placement and Employee Relations Branch, Civilian Personnel Division, McClellan Air Force Base, McClellan, California.

**Gerard V. Haigh, Monroe Stein,** and **Louise Evans** were appointed recently as postdoctoral fellows in clinical psychology at the Menninger Foundation, Drs. Haigh and Stein in the department of adult psychiatry and Dr. Evans in the department of child psychiatry. Other psychologists in the postdoctoral training program include Jerry Osterweil and Howard Shevrin who are second-year fellows in clinical psychology, and Douglas Jackson, Samuel J. Messick, and Charles M. Solley who are research fellows.

**Sydney Kessler** and **George H. Goody,** in addition to their private psychological practices, have created a psychological clinic at the North Hollywood (California) Hospital. They have been appointed to the staff of the medical hospital and as co-directors of the clinic. This is the first psychology clinic in a San Fernando (California) Valley Hospital.

**Leon P. Addis** has been appointed by the Board of Public Instruction of Dode County, Florida, as supervisor of the Psychological Services Diagnostic Center.

**Clark L. Wilson, Robert R. Mackie,** and **Donald N. Buckner** announce the establishment of Human Factors Research, Incorporated, Los Angeles. The new organization is comprised of all personnel formerly associated with Management and Marketing Research Corporation and has assumed responsibility for the psychological research being prosecuted under the MMRC name.

**Efraín Sánchez Hidalgo** represented the APA at the inauguration of Ronald C. Bauer as president of the Polytechnic Institute of Puerto Rico, on March 4, 1956. **Robert B. Morton** will represent the APA at the inauguration of Samuel Milton Nabrit as president of Texas Southern University on March 18, 1956. **Ann F. Neel** will represent the APA at the inauguration of Robert Eli Long as president of Park College on April 11, 1956. **Samuel W. Fernberger** represented the APA at the celebration of the founding of *Biological Abstracts* on February 17, 1956, at Philadelphia, Pennsylvania.

VA DEPARTMENT OF MEDICINE AND SURGERY ANNOUNCEMENTS

*Clinical Psychology Division*

**Herman Y. Efron** has transferred from the staff of VA Hospital, Louisville, Kentucky, to the staff of VA Hospital, Lyons, New Jersey.

**George M. Faibish** has transferred from the staff of VA Hospital, Downey, Illinois, to the staff of VA Hospital, Houston, Texas.

**Vernon E. Fisher** was erroneously listed in the February roster on the staff of VA Regional Office, Buffalo, New York. He has accepted a position on the staff of VA Hospital, Minneapolis, Minnesota.

**Jacob S. Nakshian,** a graduate of the VA Training Program, University of Connecticut, has been appointed to the staff of VA Hospital, Albany, New York.

**John R. Tilton,** a graduate of the VA Training Program, Michigan State University, has been appointed to the staff of VA Hospital, Battle Creek, Michigan.

**Frank G. Verdicchio** has transferred from the staff of VA Hospital, Augusta, Georgia, to the staff of VA Hospital, Richmond, Virginia.

*Counseling Psychology*

**Theodore Andreychuk,** a graduate of the VA Training Program, University of Texas, has accepted a staff position at VA Hospital, Downey, Illinois.

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**Correction.** In the December 1955 issue of the *American Psychologist* it was erroneously announced that Joseph Andriola is associate professor of psychology at the University of Oklahoma. He is associate professor in the School of Social Work and is teaching social work at the University of Oklahoma.

**Donald G. Marquis** was one of 16 new members recently appointed by Secretary of State Dulles to the U. S. Commission of Unesco.

The **APA Committee on Undergraduate Education** desires to facilitate interchange of teaching information among instructors in undergraduate psychology. It is anticipated that both convention programs and the columns of this Journal will be employed as media for this operation. All readers are invited to send to the committee chairman (Frank W. Finger, Psychological Laboratory, Peabody Hall, University of Virginia, Charlottesville, Virginia) descriptions of unusual teaching procedures, demonstrations, experiments, course syllabi, etc. with appropriate validation data if available.

**Walter Van Dyke Bingham Lecture.** The third Walter Van Dyke Bingham lecture will be given at The Ohio State University on Tuesday, April 17, 1956, by Donald G. Paterson of the University of Minnesota. The title of the lecture is, "The Conservation of Human Talent." The Walter Van Dyke Bingham lectureship was established by Mrs. Millicent Todd Bingham in memory of her husband, because of Dr. Bingham's keen interest in the discovery of talent. The lectureship honors a psychologist who has made an outstanding contribution to the advancement of this branch of personnel psychology, and also honors the institution at which the lecture is given for its contribution to this field. The first in the lecture series was given in March of 1954 by Lewis M. Terman at the University of California at Berkeley on "The Discovery and Encouragement of Exceptional Talent." The second lecture, on "The Diversity of Talent," was to have been given in March of 1955 at Columbia by L. L. Thurstone, but was omitted because of his illness and untimely death. The lecturer and the institution are each year selected by a committee appointed by the American Psychological Association.

The **Second Annual Conference on Reading** at Concordia College, Moorhead, Minnesota, has been scheduled for June 18–22, 1956. The main speakers for the five-day conference will be Emmett A. Betts, Carolyn M. Welch, and P. A. Killgallon. Inquiries may be addressed to Walther G. Prausnitz, Head, English Department, Concordia College, Moorhead, Minnesota.

The Department of Psychology and the Bureau of Special and Adult Education, The Ohio State University in cooperation with the State Department of Education offered a three-week **Workshop for School Psychologists** July 25 to August 12, 1955. Twelve school psychologists participated in the workship. Ralph Tindall, Director of Psychological Services, Milwaukee Public Schools, and Robert N. Walker, Director of Child Study Services and Guidance, Akron Public Schools, were consultants. Consultants from the State Department of Education and the Ohio State Campus representing related areas in psychology, social work, and education were utilized. Cases were presented each day by workshop participants and discussed

by the group. As part of the workship activity, a bulletin entitled "The School Psychologist, a Consultant to School and Community" was produced. Anyone wishing a copy of the bulletin may obtain it by writing to Harold R. Phelps, 321 Arps Hall, The Ohio State University, Columbus 10, Ohio.

The **Virginia Beyer Memorial Lecture** for 1956 took place at Springfield State Hospital, Sykesville, Maryland on March 4 and 5. The lecturer was Carl R. Rogers, and the topic was "Newer Concepts in Nondirective Therapy."

A **Workshop in School Psychology** will be held at the University of Minnesota under the joint sponsorship of the Institute of Child Welfare, the College of Education, and the Department of Psychology during the second summer session, July 16 to August 17, 1956. The workshop is directed toward persons now employed as school psychologists and persons with psychological training who wish to further prepare themselves for the rapidly expanding specialty of school psychology. Guest lecturer for the workship, which offers nine graduate credits, will be Susan W. Gray of the department of psychology of the George Peabody College for Teachers. Information and application forms may be obtained by writing the Workshop Coordinator, Dr. Dan C. Overlade, Institute of Child Welfare, University of Minnesota, Minneapolis 14, Minnesota.

Next summer Western Reserve University will offer the following **Rorschach Method Workshops** directed by Marguerite R. Hertz, associate clinical professor of psychology: "Introduction to the Rorschach Method," June 11–15, inclusive; "Intermediate Course in the Interpretation and Clinical Application of the Rorschach Method," June 18–22, inclusive; advanced course in the interpretation of Rorschach records of various personality and clinical groups, June 25–29, inclusive. The fee for each Workship is $40.00. Requests for information and application forms should be addressed to the Department of Psychology, Western Reserve University, 1901 Ford Drive, Cleveland 6, Ohio.

The Family Study Center of the University of Chicago will offer a workship in **Family Life Education and Evaluation** from July 9 to 27, 1956. Information about it may be obtained from Mrs. Winifred L. O'Donnell, Family Study Center, University of Chicago, 5757 Drexel Avenue, Chicago 37, Illinois.

Three summer work conferences on **Education for Leadership in a Free World** will be held next summer at Columbia-Teachers College. The three workshops will cover the topics "Improving Staff Relations," "Professional Development," and "Evaluation and Action-Research." For information write to Three Summer Work Conferences, Box 304, Columbia-Teachers College, New York 27, New York.

A conference on **Psychology in Medical Education** will be held in New York City beginning March 12 and continuing through March 14, 1956. The meetings will be attended by more than 50 invited psychologists from faculties of medical schools, as well as representatives of the American Psychological Association, the Association of American Medical Colleges, and the Veterans Administration. The program will include a resumé and discussion of a job analysis report, as well as work groups on teaching, research, clinical service, and selection and administration. Inquiries for additional information may be addressed to: Dr. Irwin J. Knopf, Chairman, Committee on Publicity and Publications, Department of Psychiatry, State University of Iowa, Iowa City, Iowa.

The **Puerto Rico Psychological Association** is planning to hold its Second Annual Meeting on April 6 and 7, 1956, at San Juan, Puerto Rico. Robert L. Thorndike will be guest speaker at the meeting.

The Twenty-Fifth Annual Meeting of the **Inter-Society Color Council** will be held at the Statler Hotel, New York City, on April 5 and 6, 1956. A two-day program has been planned with the general title, "Color Problems in the Graphic Arts."

The **Third Interamerican Congress of Psychology,** sponsored by the Interamerican Society of Psychology, was held at the University of Texas, Austin, Texas, on December 16–20, 1955. The central theme of the Congress, "The Psychology of Social Tensions," was discussed in various panels, such as "The Human Relations Laboratory for Investigations of Social Tensions," "Tension-Mapping the Psycho-social World of the School System," "Approaches to Intergroup and International Understanding," "Intercultural Tensions in Europe, the Near East and North America," Intercultural

Tensions in Mexico and the United States," "Tensions in Childhood." Several individual papers presented in the general sessions dealt with tension problems in communities and in psychotherapy. Among foreign delegates were representatives from Argentina, Canada, the Dominican Republic, Jamaica, Mexico, Panama, and Venezuela. The Anti-Defamation League of B'nai B'rith, the Hogg Foundation for Mental Hygiene, and the University of Texas gave generous financial support to the Congress Committee which offered meals, teas, receptions, and entertainment to all registered participants, as well as hotel accommodations to the foreign delegates. The United States State Department facilitated the travel of some foreign delegates and J. Manuel Espinosa, Chief of Professional Activities of the State Department's International Exchange Program gave the keynote address.

Officers of the Interamerican Society of Psychology, elected for 1956, are: president, Otto Klineberg, Columbia University; vice president, Guillermo Davila, National University of Mexico; secretary general, Werner Wolff, Bard College; treasurer, Gustave M. Gilbert, Michigan State University. The Interamerican Society of Psychology plans with its Committee on Interamerican Cooperation (chairman, H. H. Anderson), on Membership (chairman, S. Pearlman), on Publication (chairman, L. Sontag), on Research (chairman, K. Dallenbach) to further the development of interamerican psychology and education. The first monograph of the Society, *Present Day Psychology in the Americas,* written by the Society's representatives in North and Latin America, has just been issued. Contributions at the Third Congress, the translation of which were transmitted by electronic equipment, will be published in journals and monographs. Applications for membership may be directed to the Secretary-General, Werner Wolff, Bard College, Annandale-on-Hudson, New York.

The Twelfth Congress of the **International Association of Applied Psychology** was held in London on July 18 to 23, 1955. There was an attendance of 550 delegates. Meetings of the Congress were organized partly as symposia and partly as groups of individual papers related to a particular theme. There were four plenary sessions with symposia on subjects of interest to all in the field of applied psychology, and fifteen other symposia, each devoted to some special theme. In addition, there were forty individual papers. There were three evening lectures by Sir Frederic Bartlett, E. Mira y Lopez, and C. Pellizzi. The Organizing Committee of the London Congress is proposing to publish a volume of Proceedings which will contain papers in full presented at the major symposia, the opening session, and the three evening lectures. Summaries of the other papers will be included. The complete text of some of these papers will appear in one or other of the scientific journals, while the European Productivity Agency will be publishing twenty-one papers which are concerned with industrial psychology.

At the General Meeting of the Association held during the Congress, it was decided to change the title of the Association to: Association Internationale de Psychologie Appliquée—International Association of Applied Psychology. The officers of the Association were re-elected as follows: president, C. B. Frisby; past president, H. Piéron; vice president, J. Germain; general secretary, R. Bonnardel; treasurer, M. Coumétou.

The next Congress will be held in Madrid in 1958 under the presidency of José Germain. Further details about the London Congress and on the organization of the next Congress in Madrid will be given in forthcoming issues of the Bulletin of the Association.

The **VA Hospital, Albany, New York** held a meeting on November 9, 1955, on the theme "Rehabilitation of Psychiatric Patients Within a General Medical and Surgical Hospital."

The **Committee on Public Health and the Behavioral Sciences** met recently in New York City. Subcommittees from each of the disciplines presented reports on ideas for research within the public health area, and the group concluded "there is a tremendous amount of knowledge and theory presently available and applicable to solution of public health problems, but not as yet being utilized." They also concluded "there is a wide range of problems of interest to social scientists within the public health area which are not as yet being studied." The Committee will meet again April 27 at the Columbia University Faculty Club. Among those attending the last meeting were Margaret Mead, Benjamin Paul, Calvin S. Hall, Alvin Zander, George B. Darling, Alfred L. Frechette, George Rosen, John A. Clausen, Bernard Kutner, Robert Straus, Odin Anderson, and Ernest W. Gruenberg.

Officers of the **Psychological Association of Western New York** for the year 1956–1957 are: Joseph R. Sanders, president; John V. Joyce, vice-president; Thomas R. Sonne, secretary; James C. Drasgow, treasurer.

The **Arkansas Psychological Association** announces the appointment of the following officers and council for 1956: S. J. Fields, president; H. K. Moore, past president; E. Philip Trapp, president-elect; Merton F. Schmolke, secretary-treasurer; Carl Wright, conference delegate; J. V. West and Frederick Schnadt, executive council members-at-large. The 1956 committee appointments are as follows: Ethics and Professional Practices: Hardy Wilcoxon, chairman; Bartley E. Bess, Jr., and Charles Morehead. Membership: Iva Gardner, chairman; S. J. Fields and M. F. Schmolke, *ex officio*, John T. Bledsoe and Charles Jones. Nominations and Elections: H. K. Moore, chairman; Walter J. Richards and Paul Cauffiel. Legislation: Robert C. Cannon, chairman; Dolph Camp and Dean C. Andrew. Program: Philip Trapp, chairman; Norman Gale and Roscoe Dykman. Public and Interprofessional Relations: Frederick Schnadt, chairman; Donald Kausler, Jerome Schiffer, Jean Gardiner, Felman Sorsby, and Merton Schmolke. Members of the Arkansas Board of Examiners in Psychology for 1956 are: Jerome Schiffer, chairman; John Anderson, Jean Gardiner, Joseph West, secretary; and Oddist Murphree.

At the recent business meeting of the **Detroit Psychological Association,** C. M. Louttit was installed as president, William H. Knapp was elected president-elect, and James V. Tattan was elected secretary-treasurer.

The chairman of the **Psychology Section of the Virginia Academy of Science,** Gilbert Rich, has appointed a Constitutional Committee which is concerned with the formulation of a constitution acceptable both to the Virginia Academy of Science and to the members of the Psychology Section of the Virginia Academy of Science. It is contemplated that the new psychological organization will be entitled the Virginia Psychological Association and, if possible, the new organization will be affiliated with the Virginia Academy of Science. The chairman of the Constitutional Committee is William J. Morgan of Merrifield, Virginia, and its members include: Richard Henneman, Stanley Williams, Reuben Horlick, Jacob Silverberg, and Gilbert Rich, *ex officio*.

The **University of Minnesota** has announced a doctoral training program for research workers in the behavioral sciences. It will be a four-year program of integrated training for students who intend to follow research careers in one or a combination of the following behavioral sciences: economics, political science, social anthropology, psychology, or sociology. A small number of highly qualified students will be given fellowship and assistantship support; not more than ten students will be admitted per year. Students interested in applying to this program may write for information to the Graduate School of the University of Minnesota.

The **Eastern Pennsylvania Psychiatric Institute** is nearing completion in Philadelphia and expects to start receiving patients in April. It will be housed in a modern eleven-story building within the city limits and will have 250 adult and 50 children's beds. The team approach will be utilized in treatment with approximately 20 teams planned. A large portion of the staff will be allocated to research and training. The three units, Children's Unit, Out-Patient, and In-Patient will utilize approximately six psychologists in each. Kathryn Domingues is assistant director of the Psychological Service for the Children's Unit. She was formerly assistant director at the Child Psychiatric Center, Scranton, Pennsylvania. Thomas F. Johnson is assistant director for the Out-Patient Unit. He was formerly chief clinical psychologist, State Hospital, Osawatomie, Kansas. Charles M. Morris, formerly associate director, the Child Guidance Center, Easton, Pennsylvania, is director of psychological services.

The **Lafayette Clinic,** a new 145-bed psychiatric research and training center, supported by the State of Michigan and affiliated with Wayne University, has begun operation in Detroit. There will be in-patient and out-patient facilities for both adults and children. A training program for predoctoral junior fellows will begin in July, 1956. Students from American and foreign universities are invited to participate. Initial staff and academic appointments include: Henry P. David, head, division of psychology; Eli Z. Rubin, chief psychologist, children's unit; Rita Senf, staff psychologist. Further appointments are in process.

**Merrill-Palmer School** announces a new graduate and postgraduate program of intensive research training in human development and family

life. For details write to Dr. Irving Sigel, Merrill-Palmer School, 71 E. Ferry Avenue, Detroit 2, Michigan. For information on fellowships write to the Registrar at the same address.

**A Bureau of Psychological Services** has recently been established by the New York Department of Education. It will provide advice, assistance, supervision, and leadership in establishing and developing effective school psychology and school social worker services throughout the state. William A. Sivers has been appointed chief of the new Bureau.

The January issue of **Gawein, a new European psychological journal,** will contain a special review of "The position of psychological science in the different countries of Europe." The present status of psychology in sixteen European countries will be covered. On hundred copies have been reserved for American psychologists. A copy may be obtained for $1.00 from *Gawein,* c/o Miss M. A. Lombaers, Ubbergseveldweg 81, Nijmegen, Holland.

A definitive study, **Soviet Professional Manpower,** by Nicholas DeWitt, published by the National Science Foundation, summarizes two years of research on the Soviet educational system and its distribution of professional manpower. In certain fields the Soviet Union is graduating almost twice as many specialists as the U. S. Between 1928 and 1954 the Russians graduated 682,000 professionals in the engineering field, as against 480,000 in the U. S. during the same period. Soviet graduates in medicine outnumbered those in the U. S. two to one. Although Soviet higher educational institutions graduated only half as many persons in all fields as did the U. S. during the last twenty-five years, one conclusion of the study is that their "educational policies and practices have been shaped to meet the growing needs for scientific and technical manpower, with the result that from a very much smaller educational base the Soviet Union is turning out a higher number of trained specialists than is the U. S." The Soviets have a drastically small number of graduates in the humanities, whereas between 65 and 70 per cent of all U. S. graduates are in the social sciences and liberal arts. Of the two million professionals in the Soviet Union, about 42 per cent were trained for and are employed in the field of education. Soviet young people were found not to be free to change their occupations, as job placement is involuntary and strictly follows their field of training.

The National Science Foundation has also recently published three reports on current facts of science. *Federal Funds for Science* covers the fiscal years for 1954, 1955, and 1956—the last two are estimated. In 1954 expenditures for research and development were over $2 billion, but less than 7 per cent was obligated for basic research. A second publication, *Scientific Personnel Resources,* breaks down the number of 200,000 scientists in the country by field, age, and level of education. It warns that the number of college graduates completing standard requirements to teach high school science and mathematics has decreased from a high of 9,000 to an estimated 4,000. *Scientific Research Expenditures by the Larger Private Foundations* surveys the expenditures by 77 large endowed foundations during 1939, 1946, and 1953. It reveals that in 1953 expenditures by these 77 foundations totaled about 164 million dollars, less than 4 per cent of the amount spent for philanthropy in the United States, and of these 164 million dollars, 26 million were spent for scientific research. Only 43 of the 77 foundations supported scientific research.

The **Connecticut State Psychological Society** has printed an attractive special issue of their journal, the first of a series to be devoted to "psychological articles of general public interest." Six contributors discuss the present status of psychological service in Connecticut in various fields. They explain in non-technical terms what is going on in counselirg psychology in Connecticut, industrial psychology in Connecticut, psychological services in the public schools, psychologists in federal service in Connecticut, the clinical psychologist—in community services and in state hospitals—and psychologists in other state programs. Karl F. Heiser contributes the introductory article on the development of the state society.

The Vanderbilt University School of Law has announced the publication of the **Race Relations Law Reporter,** a professional magazine devoted to the impartial dissemination of the primary materials in race relations law, including much that is unavailable from other sources. It will contain the accurate and complete text of documents such as the decisions of courts, provisions of state constitutions, acts of state legislatures, ordinances of municipalities, opinions of attorneys-general, regu-

lations of state departments of education, and rulings of local boards of education. Subscription price is $2.00. For additional information write to John W. Wade, Dean, Vanderbilt University School of Law, Nashville 5, Tennessee.

How the average TV viewer can solve personal problems with an understanding of basic psychology is the subject of a new educational television series to be released by the Educational Television and Radio Center. Entitled **Not in Our Stars,** the 13-week series will be featured during the spring by the nation's educational television stations. Program host is Fred McKinney, chairman of the psychology department at the University of Missouri, who takes the viewer "to visit" a psychiatrist, a vocational counsellor, a class of deaf children, and to several hobby groups. A more detailed description of the program will appear in an article by Dr. McKinney, which is scheduled for publication in the *American Psychologist.*

The **National Institute of Mental Health,** upon recommendation of the National Advisory Mental Health Council at its June and November 1955 meetings, has awarded 42 new research grants. The total amount awarded in new grants was $674,-149. The Advisory Council also approved the continuation of 102 research grants, amounting to $1,713,272. New grants in the field of psychology were awarded as follows:

Henry K. Beecher. The effects of certain drugs on personality. Three years, first year, $15,505.

Barbara Biber and Dorothy Dinnerstein. The psychological impact of school experience. Four years, first year, $41,677.

Robert K. Burns, Emmet B. Bay, Ward C. Halstead, Robert W. Kleemeier. Criteria of aging and determinants of retirement. Two years, first year, $53,192.

Bingham Dai. Personality problems in Chinese culture. Three years, first year, $11,583.

Tamara Dembo and Heinz Werner. Psychological development in cerebral palsy. Five years, first year, $29,854.

Charles W. Eriksen. An experimental study of behavior without awareness. Three years, first year, $10,560.

Robert S. Feldman and Wladimir T. Liberson. The modification of fixated and convulsive behavior. Three years, first year, $6,143.

Jerome Fisher. Relation of physical disease to body-object cathexis. Two years, first year, $10,778.

Ernest Furchtgott. Behavioral effects of fetal X-irradiation. One year, $2,442.

Charles Y. Glock and Henry L. Lennard. Relevance of small group research to therapy. One year, $9,050.

Ruth E. Hartley. Development of concepts of women's roles. Three years, first year, $1,238.

William Fawcett Hill. Study of therapy groups through stimulated recall. Two years, first year, $12,078.

Philip W. Johnston. Effect of hearing lost on mentally retarded children. One year, $10,151.

Jacob S. Kounin and Paul V. Gump. Leader handling of deviant behavior in groups. Two years, first year, $25,443.

Herschel W. Leibowitz. Analytic studies of visual perception. Three years, first year, $7,262.

Seymour Levine and Sheldon J. Korchin. The effects of infantile trauma on adult behavior. Two years, first year, $6,815.

Donald W. MacKinnon and Jack Block. Research in affective experience. Three years, first year, $17,009.

George F. Mahl. Patients' language as expressive behavior. Two years, first year, $11,500.

Henry A. Murray. The interpretation of emotion in others. One year, $7,932.

Edwin S. Shneidman. The psychology of suicide. Three years, first year, $17,820.

Maurice F. Smith. The effects of drive reduction on food preference. Two years, first year, $4,381.

Forrest B. Tyler, Janet E. Rafferty, and Bonnie B. Tyler. Development of behavior patterns in children. Two years, first year, $25,507.

Robert D. Wirt. Follow-up study of personal and social adjustment. Two years, first year, $27,569.

Herbert F. Wright. Children's behavior in communities differing in size. Three years, first year, $21,315.

David Zeaman. Learning and transfer in mental defectives. Three years, first year, $10,995.

Axel Wenner-Gren, donor for the **Wenner-Gren Foundation for Anthropological Research,** has announced a bequest to establish a new scientific center in Stockholm, Sweden. The purpose is to encourage collaboration between foreign and Swedish research workers, by giving accommodation to

approximately 100 foreign scientists. Ten of the eighteen stories of the building will be used as exhibition and office premises, and it is planned to rent apartments to scientists at extremely low rates. Plan for the center was done by a board including Nils Bohr of Copenhagen, Siegbahn, Theorell and Nilsson of Stockholm, and Virtanen of Holland.

A postdoctoral training fellowship in general clinical psychology is offered by **The Menninger Foundation** with the support of the United States Public Health Service. Intensive experience is provided in the areas of diagnostic testing, psychotherapy, child psychology, and research. Combined USPHS and Menninger Foundation stipends total $4,200 for the first year of training and $4,800 the second year. $3,600 of the annual stipend is tax exempt. An applicant must have a PhD in clinical psychology with a minimum of one year of supervised clinical experience. To be eligible for a United States Public Health stipend, he must be a citizen of the United States and must not be receiving any other federal financial support during his period of training. For information or application forms write to Dr. Martin Mayman, Director of Psychological Training, Department of Education, The Menninger Foundation, Topeka, Kansas.

The National Training Laboratories of the National Education Association will hold its **Tenth Annual Summer National Training Laboratory** in Group Development at Gould Academy, Bethel, Maine, on June 17 to July 6, and July 22 to August 10. The National Training Laboratories offer to members of the APA one tuitional scholarship for the 1956 summer sessions of the National Training Laboratory in Group Development, to be held at Gould Academy, Bethel, Maine. The scholarship, valued at $200, can apply either to the first session (June 17 to July 6, 1956) or to the second session (July 22 to August 10, 1956). The scholarship recipient would be required to pay living expenses (board, room, gratuities, and State of Maine tax on food) of $125, for the three-week period, and travel expenses to and from Bethel, Maine. For additional information write to Gordon L. Lippitt, National Training Laboratories, 1201 Sixteenth Street N.W., Washington 6, D. C.

The **Society for the Psychological Study of Social Issues** announces a program of grants-in-aid for research on desegregation. A total of $1,000 has been made available for such awards, but no single grant will be made in excess of $500. A committee of judges has been appointed to evaluate applications; it consists of Kenneth B. Clark, Herbert Hyman, M. Brewster Smith, and Isidor Chein, chairman. Applications, in quadruplicate, specifying budgetary needs and giving sufficient detail to make possible an evaluation of the feasibility and desirability of the proposed project must be submitted to the committee chairman (Research Center for Human Relations, New York University, 21 Washington Place, New York 3, N. Y.) before June 1, 1956.

For several years the APA committees on a central office library have considered the need for and the nature of a psychological library which might be housed in the headquarters building in Washington. Several excellent plans and proposals have been considered which, for reasons of current limitations of maintenance funds, could not be adopted. The committee now has decided to proceed on a modest scale to accumulate a small, special library which might become the nucleus for a much larger future project. As of the appearance of this announcement, the committee invites all psychologists who have been, are, or will be authors of books pertaining to psychology to contribute an autographed copy to the new APA library. We are particularly desirous of receiving autographed copies of all works published since 1945, the year in which APA and AAAP were merged. Since, in addition to its immediate practical value as a reference resource, the collection will rapidly increase in historical importance, authors are encouraged to include a personal photograph mounted on the inside cover of each contribution. The book should be addressed to the Committee at the APA Central Office, 1333 Sixteenth St. N.W., Washington 6, D. C.

Committee on Library Function of the Central Office, George Wischner, Laurance Shaffer, Ilse Bry (corresponding member), Carl Murchison (corresponding member), Alice Bryan (corresponding member), John Stafford, chairman.

# Convention Calendar

**American Psychological Association:** August 30–September 5, 1956; Chicago, Illinois
*For information write to:*
Dr. Fillmore H. Sanford
1333 Sixteenth Street N.W.
Washington 6, D. C.

**Southwestern Psychological Association:** March 22–24, 1956; Dallas, Texas
*For information write to:*
Dr. Ernestine B. Bowen
Division of Mental Health
State Health Department
Austin, Texas

**Eastern Psychological Association:** March 23–24, 1956; Atlantic City, New Jersey
*For information write to:*
Dr. Gorham Lane
Department of Psychology
University of Delaware
Newark, Delaware

**American Psychosomatic Society:** March 24–25, 1956; Boston, Massachusetts
*For information write to:*
Miss Joan K. Erpf
American Psychosomatic Society
551 Madison Avenue
New York, N. Y.

**American Personnel and Guidance Association:** March 25–29, 1956; Washington, D. C.
*For information write to:*
Mr. Arthur A. Hitchcock
American Personnel and Guidance Association
1534 "O" Street N.W.
Washington 5, D. C.

**Southern Society for Philosophy and Psychology:** March 29–31, 1956; Asheville, North Carolina
*For information write to:*
Dr. Joseph E. Moore
Department of Psychology
Georgia Institute of Technology
Atlanta, Georgia

**Western Psychological Association:** March 29–31, 1956; Berkeley, California
*For information write to:*
Dr. Leona E. Tyler
Department of Psychology
University of Oregon
Eugene, Oregon

**Child Study Association of America:** April 2–4, 1956; New York City
*For information write to:*
Dr. Gunnar Dybwad
Child Study Association of America
132 East 74th Street
New York 21, New York

**Industrial Relations Conference:** April 3–4, 1956; Minneapolis, Minnesota
*For information write to:*
Director, Center for Continuation Study
University of Minnesota
Minneapolis 14, Minnesota

**Inter-Society Color Council:** April 5–6, 1956; New York City
*For information write to:*
Mr. Ralph Evans, Secretary
Inter-Society Color Council
Color Technology Division, Bldg. 65
Eastman Kodak Company
Rochester 4, New York

**Optical Society of America:** April 5–7, 1956; Philadelphia, Pennsylvania
*For information write to:*
Dr. Stanley S. Ballard
Visibility Laboratory
Scripps Institution of Oceanography
San Diego 52, California

**International Council for Exceptional Children:** April 10–14, 1956; Minneapolis, Minnesota
*For information write to:*
Mr. Edward Eugene Hussian
1201 Sixteenth Street, N.W.
Washington, D. C.

**Aero Medical Association:** April 16–18, 1956; Chicago, Illinois
*For information write to:*
Aero Medical Association
Post Office Box 26
Marion, Ohio

**Western Gerontological Society:** April 20–21, 1956; Los Angeles, California
*For information write to:*
Dr. Oscar J. Kaplan
San Diego State College
San Diego 15, California

**West Virginia Psychological Association:** April 20–21, 1956; Institute, West Virginia
*For information write to:*
Dr. Herman G. Canady
Department of Psychology
West Virginia State College
Institute, West Virginia

**American Society of Adlerian Psychology:** April 28–29, 1956; Chicago, Illinois
*For information write to:*
Dr. Irvin Neufeld
103 East 86th Street
New York 28, New York

**Southeastern Psychological Association:** April 29–May 1, 1956; Atlanta, Georgia
*For information write to:*
Dr. M. C. Langhorne
P. O. Box 2
Emory University, Georgia

**American Psychiatric Association:** April 30–May 4, 1956; Chicago, Illinois
*For information write to:*
Mr. Austin M. Davies
Room 310
1270 Avenue of the Americas
New York, N. Y.

**Midwestern Psychological Association:** May 3–5, 1956; St. Louis, Missouri
*For information write to:*
Dr. Donald W. Fiske
5728 South Ellis Avenue
Chicago 37, Illinois

**Rocky Mountain Branch of the APA:** June 11–13, 1956; Grand Teton National Park, Wyoming
*For information write to:*
Dr. Wilson J. Walthall, Jr.
Department of Psychology
University of Wyoming
Laramie, Wyoming

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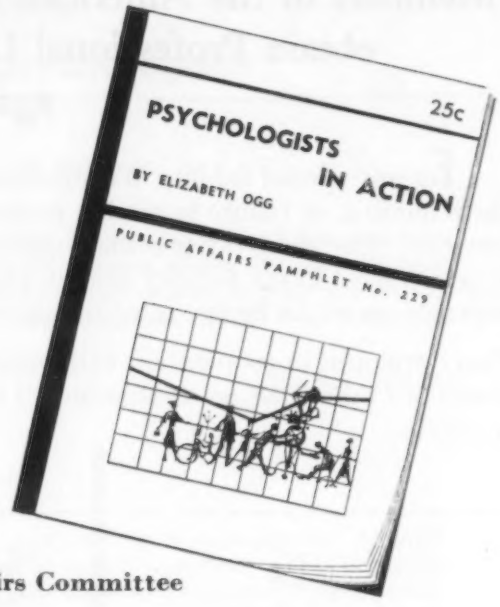


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